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OF

THE QUARTERLY JOURNAL,

No. XXV.

TO CORRESPONDENTS.

Mr. James Sayer's paper, " On the products of the action of sulphuric acid on alcohol," reached us too late for insertion in this Number, and is therefore disposed of according to his directions.

The Review, commencing with the proverb " It is an ill wind that blows nobody good," we decline inserting.

We are much obliged by Mr. John Vaillant's communication on Secret Writing; and to X. for a letter on the same subject.

Our medical correspondent at Manchester will find the information he desires in our last Number, under the head of *Foreign Science.*

The Observations on the use of arsenic in the cure of intermittent fevers, we think excellent, but too exclusively medical for this Journal; we will, therefore, either return the MS. to the author, when his address is known, or transmit it to a Medical Journal.

P + p, Typographus, a letter dated Naples, January 1, with a signature which we cannot decipher, and " Amicus Scientiarum," are under consideration.

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OF

THE QUARTERLY JOURNAL,

N°. XXVI.

TO CORRESPONDENTS.

No. XXI. of this Journal, containing "a Review of Doctor Thomson's System of Chemistry," has been reprinted.

We are much indebted for the advice of "a Member of the College of Surgeons;" it shall be strictly followed.

Mr. Bowdich's paper on the Measurement of the Progress of the Moon, with a Sextant, or Reflecting Circle, is postponed on account of the extent of our astronomical communications in the present Number.

The communications of our Manchester correspondent are always acceptable; he will see we have availed ourselves of his information.

O, is in error respecting our Review department. We regret that the article on Messrs. Conybeare and Phillips's Geology did not reach us sooner, as we could have made room for it; we trust, however, that the delay will enable our correspondent to perform his promise; at all events, we shall take an opportunity, in our next publication, of recommending that excellent work to our readers.

"Confusion worse Confounded," will observe that he has been anticipated.

We beg to decline the aid of "Speculator Mediolensis."

An analysis of Mr. G. Mantell's work on the Geology of Sussex, in an enclosure, signed M. G. S., did not, by some accident, arrive so soon as from its date, it should. We again request leave to remind our Correspondents, that articles intended for insertion should reach us at least three weeks before we publish.

Numismaticus shall be attended to whenever he chooses to appear in *propriâ personâ*.

THE

QUARTERLY JOURNAL,

April, 1822.

Art. I. *On the Composition and Manufacture of Chloride or Oxymuriate of Lime, (commonly called Bleaching-Powder,) and on the Atomic Weight of Manganese. By* Andrew Ure, M.D., F.R.S., *Professor of the Andersonian Institution in Glasgow, &c.*

The pulverulent oxymuriate, or chloride of lime, a combination equally interesting to science, and important to the arts, was first invented by Charles Tennant, Esq., an ingenious and extensive manufacturing chemist in Glasgow. His patent right to the exclusive preparation of oxymuriate of lime, was granted in 1799. On this curious compound several memoirs have been written, besides the specification of the process for making it, given in the patent. The first author, who treated the subject methodically, was Mr. Dalton, who wrote a paper on it in the first Number of the Annals of Philosophy; and another, which forms the second article of the second volume of that periodical work. His first memoir is occupied with a specimen of bleaching-powder from Mr. Tennant, which was possibly somewhat altered by carriage to Manchester, and keeping. In the second memoir, he examined a portion of oxymuriate of lime, recently prepared by Dr. Henry, by passing chlorine over proto-hydrate of lime, till it was saturated with the gas. Mr. Dalton found this chloride to consist of 23 oxymuriate of lime, 38 lime, and 39 water in 100 parts. "Referring now the "atomic system," says this ingenious philoso-

pher, " in order to have a clear view of these compounds of oxymuriatic acid and lime, it appears that the dry oxymuriate of lime, or, as it should be called, *hyperoxymuriate,* consists of 1 atom acid, 2 of lime, and 6 of water; namely,

1 of oxymuriatic acid . .	29	or 23.2
2 of lime, ...	48	or 38.4
6 of water, ...	48	or 38.4
	125	100.0

When the salt is dissolved in water, one half of the lime is precipitated, and the liquid contains a solution of oxymuriate of lime, the proportions of the elements of acid and base being then,

1 atom acid, . . .	29	or 54.7
1 atom lime, ...	24	or 45.3
	53	100.0 *."

These atomic numbers of Mr. Dalton being divided by 7, are reducible to the oxygen *radix.* As the means of analysis, he employed, in preference, a solution of the green sulphate of iron, adding it to a given weight of the *oxymuriate,* as long as any smell of chlorine was perceptible, or till the power of the chlorine was consumed in converting protoxide of iron into peroxide. " If the sulphate is deficient," says he, " a strong smell of oxymuriatic acid accompanies the mixture; whence more sulphate must be added, till the mixture, on due agitation, ceases to emit the fumes of oxymuriatic acid : if too much sulphate is put in, then more of the acid liquor," (oxymuriate of lime solution,) " must be added by degrees, till its peculiar odour is developed †." Dr. Thomson says that he tried this test, but found it unsatisfactory. I also tried it, and found it not only unsatisfactory, but highly insalubrious ; the application of the nostrils to the mixture, in order to ascertain the neutral point, necessarily causing the inhalation of chlorine. In the sequel of his paper, Mr. Dalton advises the bleacher to use a tri-hydrate of lime ; that is, a compound of nearly equal

* *Annals of Philos.* II. p. 7. † *Idem,* p. 18.

weights of lime and water, instead of the proto-hydrate. This, however, would not, I believe, answer well in practice on the large scale, as the surface of the lime would become somewhat coherent, and prevent the interior from readily receiving its fair dose of chlorine, without an inconvenient degree of manipulation. The distant bleacher would likewise grudge to pay for the carriage of this *extra* water. Mr. Dalton likewise remarks, that " In whatever state we procure the oxymuriate of lime," (of commerce,) " it is always found to be accompanied by a portion of muriate of lime; this portion, too, increases with the age of the oxymuriate, and is furnished at its expense. It becomes a primary object of analysis, then, to ascertain how much of any given specimen is muriate, and how much oxymuriate, especially as the former is of no use in effecting the purposes to which the latter is applied *." His analytical conclusion, from the experiments on Mr. Tennant's salt, is the following: " Hence we may infer, that this is the saturation which is produced from the process of making dry oxymuriate of lime; namely when atom of the acid is combined with two atoms of lime: so that the salt may be denominated the *sub-oxymuriate* of lime. When dissolved in water, one half of the lime is deposited, and a solution of simple oxymuriate is obtained †."

In the second paper, on Dr. Henry's oxymuriate, where he shews that the muriate is an accidental, and not an essential, concomitant of the other, we find another method of analysis proposed. " It is to dissolve a known weight of the oxymuriate in a small portion of water; then put the liquid into a graduated tube over mercury, and discharge the gas by an acid; it may thus be measured, and the quantity retained by the liquid may be estimated at twice the bulk of the liquid nearly. The gas may, I find, be kept in the tube under these circumstances for a week, without losing more than 40 or 50 measures" (out of 170 measures) " of its volume; so that its combination with mercury is slow, if not agitated. Upon the

* *Annals of Philos.* I. p. 16. † *Idem*, p. 19.

whole, however, I prefer the green sulphate test for precision *." But oxymuriate of lime is a compound not very soluble in water, as we shall afterwards see; and therefore it will be imperfectly taken up by "a small quantity of water." The plan, however, of separating the chlorine by an acid, is good, and, when practised in a particular way, which will be described in the sequel, affords the best and readiest mode of analysis. As to his preference of the copperas test, I can only say that Mr. Dalton's olfactory nerves are more tractable than mine. On the whole, we must regard these two papers as highly creditable to this celebrated chemist. His methods of research display his characteristic ingenuity, and his conclusions are such, that subsequent investigators have been content to repeat them as their own.

The next occasion on which I find the subject of chloride of lime discussed, is in the *Annales de Chimie et de Physique*, for April 1818, where we meet with a good practical paper, by J. J. Welter. His mode of examination was, to try how much of a dilute solution of indigo, in sulphuric acid, could be deprived of its colour by a certain weight of a saturated chloride of lime; and knowing, by previous experiments, the quantity of chlorine adequate to the same effect, he thence could infer the quantity of chlorine condensed in the hydrate of lime. From these experiments, which seem to have been conducted with as much precision as the method of discoloration admits, he concludes that the saturated combination of lime with chlorine, is a demi-chloride; that is, a compound of 46.78 (an atom of hydrate of lime) $+\frac{44.1}{2}=22.05$, (half a proportion of chlorine). By the same means he is led to infer, that, when lime or potash is added to water impregnated with a known quantity of chlorine, the resulting chloride is free from chlorate, because as much indigo colour is destroyed by it as the free chlorine could have done. "On pouring water on the above demi-chloride," says M. Welter, "a partition takes place; the water dissolves all the chlorine along with some lime, and what remains is the

* *Annals of Philos.* II. p. 7.

hydrate of lime. It is very probable that the soluble combination is a neutral chloride, containing only half the lime of the demi-chloride." M. Welter's instructions for the use of indigo, as a test of bleaching-powder, are judicious, and shall be adverted to afterwards. I do not believe in the truth of this atomic partition, which M. Welter repeats after Mr. Dalton. I find that 1 part of commercial chloride of lime, when well triturated with 19 parts of water, leaves a notable quantity of chlorine in the undissolved part, while the solution does not consist of an atomic chloride.

More recently Dr. Thomson has published two papers on chloride of lime. The first seems to have been very hastily got up, and is only remarkable for the employment of nitrate of silver, to analyze a salt of commerce, which notoriously consists of the chloride of lime and chloride of calcium; and where the great object is, to determine the proportion of each. Now, as chloride of calcium, which is useless to the bleacher, yields with nitrate of silver, a more abundant precipitate than chloride of lime, it is evident that the worse the bleaching salt becomes, the better it would appear to be, by Dr. Thomson's test. In the same memoir * Dr. Thomson says, "Probably unslacked lime might be united with chlorine, if its temperature could be kept low." Now, in a paper which I published in his *Annals* for September 1817, it is stated, that, when carbonic acid gas, and chlorine, are subjected to dry quicklime at ordinary temperatures, there is no condensation, but that they both unite very readily with the calcareous hydrate †. In fact, when chlorine is submitted to the hydrate, the temperature rises; but there is no heat produced on its admission to dry lime, because there is no chemical action.

Dr. Thomson's second dissertation is much more elaborate than the first, displaying sounder views of analysis, and considerable experimental address. The subject of this research was a sample of bleaching-powder, manufactured at Belfast, and which, he thought, had been modified by the carriage. "I

* *Annals of Phil.* XIII., p. 185, for March 1819. † *Idem*, X., p. 213.

am disposed," says he, "to ascribe this surplus of water to some accidental exposure of the bleaching-powder to moisture during a voyage from Belfast, where it was manufactured, to Glasgow, where I analyzed it *." His new method of analysis, which indeed had been indicated in his preceding paper, consisted in subjecting the bleaching-powder to heat in a glass retort, placed in a sand-bath. He thus converted the chloride of lime into chloride of calcium, with the disengagement of oxygen. This gas being received into graduated jars, filled with water in the pneumatic trough, indicates by its volume the quantity of chlorine employed in its extrication; for each volume of oxygen corresponds, in this case, to two volumes of chlorine. Hence, estimating the volume of chlorine present at double the volume of oxygen evolved, and converting it into weight, at the rate of 100 cubic inches to 76¼ grains, the proportion of chlorine associated with the calcareous hydrate, comes to be known. On this process M. Gay Lussac observed, that if the chloride of lime by any means passes into chlorate and muriate, then the same bulk of oxygen would still be afforded, though now the powder would have entirely lost its bleaching power. It is doubtful, however, if any chlorate of lime be formed in this way, though common muriate is progressively generated, as Mr. Dalton justly remarked. The matter which remains in the retort, consists of quicklime and chloride of calcium, the latter of which, being separated by its superior solubility in water, is evaporated to dryness and weighed. His analytical conclusions are thus stated: "The true constitution of the bleaching-powder, reduced to 100 parts, is as follows:

Subbichloride of lime (suboxymuriate of Mr. Dalton	51.91	{ 20.31 chlorine 31.60 lime
Muriate of lime	15.46	
Water	27.86	
Uncombined lime	4.77	
	100.00	

* *Annals of Phil.* XV., p. 405.

" So that rather more than half of the powder consisted of pure subbichloride of lime, while the remainder consisted of matter of no efficacy for bleaching *." The specimen thus examined he regards as having possessed extraordinary bleaching strength, " most probably owing to its having been taken from the surface of the lime exposed on the bottom of the receiver, which is always much nearer a state of saturation than any other portion of the powder †." " But I have likewise," adds he, " subjected to analysis a quantity of bleaching-powder, which I have reason to believe constitutes a pretty fair specimen of the average strength at which this powder is usually exposed to sale. We find it constituted as follows :—

Subbichloride of lime	36.52	{ 14.29 chlorine 22.23 lime
Muriate of lime	18.50	
Water	16.93	
Uncombined lime	28.05	
	100.00 ‡."	

Mr. Dalton's first, or commercial oxymuriate of lime, consisted of,

Suboxymuriate of lime	44.5	{ 14.5 oxym. acid 30.0 lime
Muriate of lime	13.5	
Water	42.3	
	100.0	

His second, prepared by Dr. Henry, contained, he says, no muriate of lime. It consisted, as formerly stated, of

Oxymuriatic acid	23.2
Lime	38.4
Water	38.4
	100.0

* *Annals of Phil.* XV. p. 407. † *Ibidem, loco citato.*

‡ *Ibidem*, p. 408.

Now, surely Mr. Dalton's own atoms should agree together, or the constitution of what he reckons a suboxymuriate should be a definite proportion. Yet his first and second oxymuriates (exclusive of muriate and water) seem sufficiently discordant; for 23.2 : 38.4 :: 14.5 : 24, and not 30, as he has given it. Dr. Thomson has accommodated his numbers to suit the atomic theory, and has thrown off, for that purpose, the suitable quantity of "uncombined lime." The great error, in my apprehension, attending all these statements, is occasioned by the ultra-atomical notions of the authors. They offer no evidence whatever, that when chlorine is presented to pulverulent slaked lime, 22.23 parts of this alkaline base do attach themselves to 14.29 parts of chlorine, to form 36.52 of a subbichloride, while there remains in the mixture 28.05 of lime, equally greedy of chlorine, and yet altogether deprived, by their companion particles, of that energetic element. Dr. Thomson's last statement above is built on this foundation. I believe, on the contrary, and hope to make it appear presently, that all the particles of hydrate of lime attach to themselves a quantity of chlorine, relative to the proportion and pressure of the gas, up to their saturating limit, which seems to be one atom of chlorine for one of tri-hydrate of lime, and that there is, therefore, no mixture of an atomic subbichloride, and of free lime, in the powder.

But before relating my own experiments, I must advert for a moment to M. Grouvelle's "*Researches on the Combinations of Oxides with Chlorine, Iodine and Cyanogen*," inserted in the *Annales de Chimie et de Physique*, for May 1821. This gentlemen asserts, that M. Welter is the first, to his knowledge, who occupied himself with the analysis of chloride of lime. But we have shewn above, that M. Welter's researches are five years posterior to those of Mr. Dalton, who had also anticipated all the atomical inductions of the French chemists. M. Grouvelle's mode of examining the chloride of lime, seems to me singu-

* *Annals of Phil.* I, p. 19.

larly unskilful, and can bear no comparison with Dr. Thomson's last method, to which he does not allude, though he animadverts on the first. "I treated," says he, "the chloride of lime at a gentle heat, with pure potash, prepared by alcohol. I evaporated and calcined the salt produced, in order to decompose the chlorate, and I precipitated by nitrate of silver the chlorine of the chloride of potassium. The lime was converted into sulphate. In another experiment I calcined immediately the chloride of lime, and precipitated by nitrate of silver. There was only a small quantity of chlorine disengaged during the calcination. The results obtained gave for the composition of the subbichloride of lime,

Hydrate of lime,	1 atom . .	936.22	67.914
Chlorine,	1 atom . .	442.65	32.086
		1379.57	100.000

And for that of the dissolved neutral chloride;

Hydrate of lime,	1 atom	51.416
Chlorine,	2 atoms	48.584
		100.000"

On such procedure as this, it is superfluous to comment. The sequel of his paper seems to shew, that he is not aware of the solution of chloride of lime passing into muriate and carbonate by long ebullition, even to dryness; a treatment to which he subjected it.

In the researches which I have made, at many different times, on the nature of the chloride of lime, I have generally sought to combine the information flowing from both synthesis and analysis; that is, I first converted a known portion of hydrate of lime into bleaching-powder, and then subjected this to analysis. Among the results of experiments in my note-book of 1815, I find the following: 500 grains of unslaked quicklime, in fine powder, from Carrara marble, were exposed in a glass globe to a copious stream of chlorine, (previously passed through a little cold water,) for four days. The increase of weight was noted from time to time, and was found, at the

end of that period, to be only 30 grains, which subsequent examination shewed to be due to a little hydrated chloride; the few grains of water requisite having been derived from the great body of undried gas which had been transmitted. In May 1817, an experiment is recorded, in which 400 grains of a hydrate of Carrara lime, equivalent to 291.28 grains of dry lime, were exposed for two days to a stream of chlorine, washed in water of 50°, and refusing to absorb more gas, were found heavier by 270.5 grains. Supposing this augmentation to be chlorine, we shall have the composition of the powder, by the synthetic mode, as follows:—

Chlorine		. . . . .	40.34
Dry lime	43.46	Hydrate .	59.66
Water .	16.20		
			100.00

This powder was analyzed, by acting on a given weight of it with dilute muriatic acid, in a pear-shaped glass vessel. Care was taken to remove the whole disengaged chlorine, without letting any liquid escape. The lime was converted into carbonate, by a solution of carbonate of ammonia. The following are the results of two independent analytical experiments:

	1st Experiment.	2d Experiment.
Chlorine evolved	40.60	39.40
Lime	42.27	42.22
Water	17.13	18.38
	100.00	100.00

I have reason to believe the second experiment the more correct of the two, and if the synthetic result be compared with it, we are led to infer that the very great body of undried chlorine passed over the lime had deposited two per cent. of water. By other experiments I satisfied myself, that dilute muriatic acid expelled nothing but pure chlorine, for the whole gas disengaged is absorbed on agitation with mercury. It does not appear possible to reconcile the above chlorides to a definite atomic constitution. The following experiments were made with much care last spring:

200 grains of the atomic proto-hydrate of pure lime were put into a glass globe, which was kept cool by immersion in a body of water at 50°. A stream of chlorine, after being washed in water of the same temperature in another glass globe, connected to the former by a long narrow glass tube, was passed over the calcareous hydrate. The globe with the lime was detached from the rest of the apparatus from time to time, that the process might be suspended as soon as the augmentation of weight ceased. This happened when the 200 grains of hydrate, containing 151.9 of lime, had absorbed 130 grains of chlorine. By one analytical experiment it was found, that dilute muriatic acid expelled from 50 grains of the chloride, 20 grains of chlorine, or 40 per cent. ; and by another, from 40 grains, 16.25 of gas, which is 40.6 per cent. From the residuum of the first, 39.7 grains of carbonate of lime were obtained by carbonate of ammonia ; from that of the second, 36.6 of ignited muriate of lime. The whole results are therefore as follows :

	Synthesis.	1st Analysis.	2d Analysis.	Mean.
Chlorine	39.39	40.00	40.62	40.31
Lime	46.00	44.74	46.07	45.40
Water	14.60	15.26	13.31	14.28
	100.00	100.00	100.00	100.00

Though the heat generated by the action of the dilute acid has carried off in the analytical experiments a small portion of moisture with the chlorine, yet their accordance with the synthetic experiment is sufficiently good to confirm the general results. The above powder appears to have been a pure chloride, without any mixture of muriate. But it exhibits no atomic constitution in its proportions.

To 200 grains of that hydrate of lime 30 grains of water being added, the powder was subjected to a stream of chlorine in the above way, till saturation took place. Its increase of weight was 150 grains. It ought to be remarked, that in this and the preceding experiment there was no appreciable pneumatic pressure employed, to aid the condensation of the chlorine. In the last case, we see that the addition of 30 grains of

water has enabled the lime to absorb 20 grains more of chlorine, being altogether a quantity of gas nearly equal to that of the dry lime. Thus an atom of lime seems associated with $\frac{7}{8}$ of an atom of chlorine. Analysis by muriatic acid confirmed this composition. It gave,

Chlorine .	39.5=51.8 cubic inches.
Lime .	39.9
Water .	20.6
	100.0

I next exposed some of this powder to heat in a small glass retort, connected with the hydro-pneumatic trough. Gas was very copiously disengaged, at a temperature far below ignition, the first portions coming off at the heat of boiling water, .100 measures of the collected gas being agitated with water at 50° F., 63 measures were absorbed, and the remaining 37 measures were oxygen, nearly pure. The smell of the first evolved gas was that of chlorine, after which the odour of euchlorine was perceived, and latterly the smell nearly ceased as the product became oxygen. Having thus ascertained the general products, I now subjected to the same treatment 100 grains of the same powder (that last described,) in a suitable apparatus; 30 cubic inches of gas were obtained from it, in a series of glass cylinders, standing over water at 50°. The first received portion was chlorine, nearly pure, but towards the end, when the heat approached, or was at, ignition, oxygen became the chief product. The residuary solid matter yielded to water a solution of muriate of lime, containing 30 grains of the dry salt, equivalent to about 15 of lime. But the chloride, both by synthesis and analysis, seemed to contain in 100 grains, 51.8 cubic inches of chlorine, (corresponding to 25.9 of oxygen,) with 39.9 of lime. Thus the volume of the evolved gas proves independent of other considerations, that a considerable portion of chlorine came off, without dislodging the oxygen from the calcium; and as in subsequent experiments this volume was found to vary with the strength of the powder, and the mode of heating it, this method of analysis becomes altogether nugatory and

delusive. The truth of this conclusion will still further appear, on reflecting that an uncertain portion of chlorine is condensed in the water of the trough, and that most probably a little euchlorine is formed at the period when the gaseous product passes from chlorine to oxygen. Thus, of the 39.9 grains of lime present in the chloride, 24.9 seem to have merely parted with their chlorine, while the other 15 lost their oxygen, equivalent to $12\frac{2}{3}$ cubic inches, or 4.3 grains, and the remaining 10.7 of calcium, combined with 19.3 of chlorine to constitute the 30 grains of ignited muriate of lime. But 19.3 grains of chlorine form 25.3 cubic inches; hence $51.8-25.3=26.5$ is the volume of chlorine disengaged by the heat, to which, if we add $12\frac{2}{3}$ cubic inches of oxygen, the sum 39.16 is the bulk of gas that should have been received. The deficiency of 9.16 cubic inches is to be ascribed to absorption of chlorine (and perhaps of euchlorine,) by the water of the pneumatic trough. In the above case, about one-half of the total chlorine came off in gas, and the other half combined with the basis of the lime, to the exclusion of its oxygen. I have observed that the proportion of chlorine to that of oxygen given off by heat, increases, as one may naturally imagine, with the strength of the bleaching-powder. When it is very weakly impregnated with chlorine, as is the case with some commercial samples, then the evolved gas consists in a great measure of oxygen.

Before proceeding to describe the manufacture of oxymuriate of lime on the great scale, the average condition of the product in the market, and the most convenient means of ascertaining its bleaching quality, I shall beg leave to make a few theoretical remarks on the state of chemical combination, between the lime and the chlorine, in the above bleaching powder. We have seen that the atomic hydrate of lime, with a slight pneumatic pressure, absorbed in the first experiment 33 parts of chlorine to 35.5, = the prime equivalent of lime: in the second experiment it absorbed, without pressure, 30.4; and in the third, by the aid of 13 per cent. of more water in the hydrate, 35.2 of chlorine. But if we add as much water to the lime as to constitute a trihydrate, that is, for 100 parts of lime 95 of water,

and subject this hydrate at the temperature of 50° to a stream of chlorine, we can without difficulty condense, on 35.5 of lime, 45 of chlorine, and even somewhat more. Now this proportion should seem to form the true state of atomic saturation and repose, for just as much chlorine is present as is capable of displacing the whole of the oxygen from the calcium, and converting the bleaching powder entirely into muriate of lime. The intimate union, however, existing between the calcium and oxygen, and the consequent close contact of their molecules, must give the latter element a chemical advantage, so to speak, over the more distant molecules of the chlorine, which seems loosely clustered round the hydrate by the joint affinities of the water and lime. It is owing to this looseness and feebleness of combination which increase with the degree of impregnation, that the slightest heat is then apt to restore elasticity to the *attached* chlorine, as we have seen. In proportion, however, as the mass of the hydrate increases, relative to the chlorine, it exercises a more powerful attraction, draws the chlorine apparently into closer combination, whence the adhesion of the oxygen is impaired. For the atoms to take a new *definite* arrangement to the subversion of an existing one, there is required a certain energy of attraction, and that energy seems to be counteracted in the present instance by the repulsion between the two vitreo-electric elements, oxygen and chlorine, while the feebler affinity of the former for calcium is compensated by proximity of contact, and thus it is enabled to hold its place at low temperatures. Heat exalts the affinities between calcium and chlorine, and at the same time lends elasticity to the oxygen. It is that feebleness of affinity between the constituents of chloride of lime which leaves the quantities of each indefinite, and makes it resemble rather a mixture, (or at most a saline solution,) than a true atomic compound. It is in fact as indeterminate in its proportions as it is unstable in its equilibrium.

Of the Manufacture of Bleaching Powder.

A great variety of apparatus has been at different times con-

trived for favouring the combination of chlorine with slacked lime for the purposes of commerce. One of the most ingenious forms was that of a cylinder, or barrel, furnished with narrow wooden shelves within, and suspended on a hollow axis, by which the chlorine was admitted, and round which the barrel was made to revolve. By this mode of agitation, the lime-dust being exposed on the most extensive surface, was speedily impregnated with the gas to the requisite degree. Such a mechanism I saw at MM. Oberkampf and Widmer's celebrated *fabrique de toiles peintes*, at Joüy, in 1816. But this is a costly refinement, inadmissible on the largest scale of British manufacture. The simplest and, in my opinion, the best construction for subjecting lime-powder to chlorine, is a large chamber eight or nine feet high, built of siliceous sandstone, having the joints of the masonry secured with a cement composed of pitch, rosin, and dry gypsum in equal parts. A door is fitted into it at one end, which can be made air-tight by stripes of cloth and clay lute. A window in each side enables the operator to judge how the impregnation goes on by the colour of the air, and also gives light for making the arrangements within at the commencement of the process. As water-lutes are incomparably superior to all others, where the pneumatic pressure is small, I would recommend a large valve, or door, on this principle to be made in the roof, and two tunnels of considerable width at the bottom of each side wall. The three covers could be simultaneously lifted off by cords passing over a pulley. without the necessity of the workman approaching the deletereous gas, when the apartment is to be opened. A great number of wooden shelves, or rather trays, eight or ten feet long, two feet broad, and one inch deep, are provided to receive the riddled slacked lime, containing generally about 2 atoms of lime to 3 of water. These shelves are piled one over another in the chamber, to the height of five or six feet, cross-bars below each keeping them about an inch asunder, that the gas may have free room to circulate over the surface of the calcareous hydrate.

The alembics for generating the chlorine, which are usually

nearly spherical, are in some cases made entirely of lead, in others, of two hemispheres joined together in the middle, the upper hemisphere being lead, the under one cast-iron. The first kind of alembic is enclosed for two-thirds from its bottom in a leaden or iron case, the interval of two inches between the two being destined to receive steam from an adjoining boiler. Those which consist below of cast-iron, have their bottom directly exposed to a very gentle fire; round the outer edge of the iron hemisphere a groove is cast, into which the under edge of the leaden hemisphere sits, the joint being rendered air-tight by Roman or patent cement*. In this leaden dome there are four apertures, each secured by a water-lute. The first opening is about ten or twelve inches square, and is shut with a leaden valve, with incurvated edges, that sit in the water-channel at the margin of the hole. It is destined for the admission of a workman to rectify any derangement in the apparatus of rotation, or to detach hard concretions of salt from the bottom. The second aperture is in the centre of the top. Here a tube of lead is fixed, which descends nearly to the bottom, and down through which the vertical axis passes, to whose lower end the cross bars of iron, or of wood, sheathed with lead, are attached, by whose revolution the materials receive the proper agitation for mixing the dense manganese with the sulphuric acid and salt. The motion is communicated either by the hand of a workman applied from time to time to a winch at top, or it is given by connecting the axis with wheel work, impelled by a stream of water, or a steam-engine. The third opening admits the syphon-formed funnel, through which the sulphuric acid is introduced; and the fourth is the orifice of the eduction-pipe.

Manufacturers differ much from each other in the proportion of their materials for generating chlorine. In general, 10 cwt. of salt are mixed with from 10 to 14 cwt. of manganese, to which mixture, after its introduction into the alembic, from 12

* A mixture of lime, clay, and oxide of iron, separately calcined, and reduced to a fine powder. It must be kept in close vessels, and mixed with the requisite water when used.

to 14 of sulphuric acid are added in successive portions. That quantity of oil of vitriol must, however, be previously diluted with water, till its specific gravity becomes about 1.5. But, indeed, this dilution is seldom actually made, for the manufacturer of bleaching-powder almost always prepares his own sulphuric acid for the purpose, and therefore carries its concentration no higher in the leaden boilers than the density of 1.65, which, from my table of sulphuric acid, indicates 1-4th of its weight of water, and therefore 1-3d more of such acid must be used.

The fourth aperture, I have said, admits the eduction pipe. This pipe is afterwards conveyed into a leaden chest, or cylinder, into which all the other eduction pipes also terminate. They are connected with it simply by water-lutes, having a hydrostatic pressure of 2 or 3 inches. In this general *diversorium* the chlorine is washed from adhering muriatic acid, by passing through a little water, in which each tube is immersed, and from this the gas is led off by a pretty large leaden tube, into the *combination room*. It usually enters in the top of the ceiling, whence it diffuses its heavy gas equally around.

Four days are required, at the ordinary rate of working, for making good marketable bleaching-powder. A more rapid formation would merely endanger an elevation of temperature, productive of muriate of lime, at the expense of the bleaching quality. But skilful manufacturers use here an alternating process. They pile up, first of all, the wooden trays only in alternate shelves in each column. At the end of two days the distillation is intermitted, and the chamber is laid open. After two hours the workman enters, to introduce the alternate trays covered with fresh hydrate of lime, and at the same time rakes up thoroughly the half-formed chloride in the others. The door is then secured, and the chamber, after being filled for two days more with chlorine, is again opened, to allow the first set of trays to be removed, and to be replaced by others containing fresh hydrate, as before. Thus the process is conducted in regular alternation; thus, to my knowledge, very superior bleaching-powder is manufactured, and thus the chlorine may be suffered to enter in a pretty uniform stream. But for this

judicious plan, as the hydrate advances in impregnation, its faculty of absorption becoming diminished, it would be requisite to diminish proportionately the evolution of chlorine, or to allow the excess to escape, to the great loss of the proprietor, and, what is of more consequence, to the great detriment of the health of the workmen.

The manufacturer generally reckons on obtaining from one ton of rock-salt, employed as above, a ton and a half of good bleaching powder. But the following analysis of the operation will shew, that he ought to obtain two tons.

Science has done only half her duty, when she describes the best apparatus and manipulations of a process. The *maximum* produce should be also demonstrated, in order to shew the manufacturer the perfection which he should strive to reach, with the *minimum* expense of time, labour, and materials. For this end I instituted the following researches. I first examined fresh commercial specimens of bleaching-powder; 100 grains of these, afforded from 28 to 22 grains of chlorine. This is the widest range of result, and it is undoubtedly considerable; the first being to the second, as 100 to 78.6. The first yielded, by saturation with muriatic acid, 82 grains of chloride of calcium, equivalent to about 41 of lime; it contained besides 26 *per cent.* of water, and a very little common muriate ready formed. On heating such powder in a glass apparatus, it yielded at first a little chlorine, and then oxygen tolerably pure. The bulk of chlorine did not exceed one-tenth of the whole gaseous product. Of the recently prepared powder of another manufacturer, 100 grains were found to give, by solution in acid, 23 grains of chlorine, and there remained, after evaporation and gentle ignition, 92 grains of muriate of lime, equivalent to about 46 of lime. Supposing this powder to have been nearly free from muriate, (and the manufacturers are anxious to prevent the deliquescent tendency which this introduces,) we should have its composition as follows:

Chlorine .	23	. . . . 3.5
Lime . .	46	one atom $3.5 \times 2 = 7.0$
Water . .	31	
	100	

This powder being well triturated with different quantities of water at 60°, yielded filtered solutions of the following densities at the same temperature:

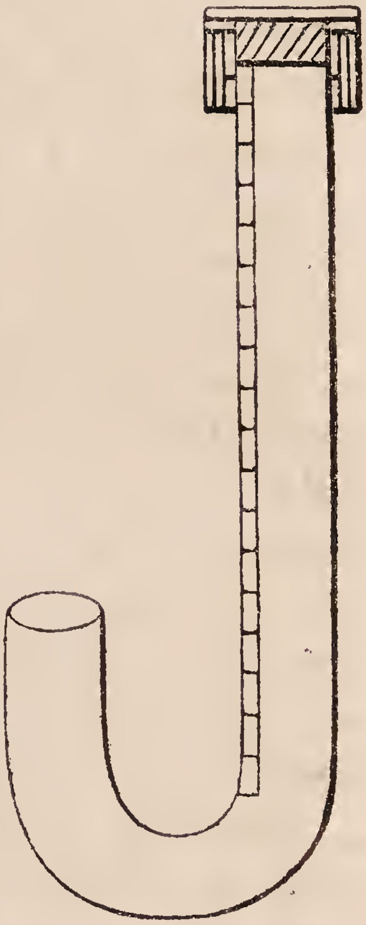
95 water	+ 5 bleaching powder.	Spec. gravity	1.0245
90	+ 10		1.0470
80	+ 20		1.0840

The powder left on the filter, even of the second experiment, contained a notable quantity of chlorine, so that the chloride is but sparingly soluble in water; nor could I ever observe that partition occasioned by water, in the elements of the powder, of which Mr. Dalton and M. Welter speak. Of the solution 80+20, 500 grains, apparently corresponding to 100 grains of powder, gave off, by saturation with muriatic acid, 19 grains of chlorine, and the liquid, after evaporation and ignition, afforded 41.8 grains of chloride of calcium, equivalent to 21 of lime. Here 4 per cent. of chlorine seem to have remained in the undissolved calcareous powder, which, indeed, on examination, yielded about that quantity. But the dissolved chloride of lime consisted of 19 chlorine to 21 lime; or of 4.5 at atom of the former, to almost exactly 5, (which is no atomic proportion,) of the latter. The two-thirds of a grain of lime, existing in lime-water, in the 500 grains of solution, will make no essential alteration on the statement. Now the above bleaching-powder must have contained very little muriate of lime, for it was not deliquescent. Being thus convinced, both by examining the pure chloride of my own preparation, as well as that of commerce, that no atomic relations are to be observed in its constitution, for reasons already assigned; I ceased to prosecute any more researches in that direction. When we are desirous of learning minutely the proportion between the chloride and muriate of lime in bleaching-powder, pure vinegar may be used as the saturating acid. Having thus expelled the chlorine, we evaporate to dryness, and ignite, when the acetate of lime will become carbonate, which will be separated from the original muriate, by solution and filtration. Or if it be feared that some muriatic acid may result, from the action of chlorine on the hydro-carbonous base

of vinegar, carbonic acid gas may supply its place. A large glass flask being filled with this gaseous acid, 20 or 50 grains of bleaching-powder may be introduced, and well agitated in the gas, 10 grains of lime take 17.2 cubic inches of carbonic acid, equal to 8 grains in weight. Therefore if 20 grains of lime, be present in 50 grains of the chloride-part of the bleaching-powder, (exclusive of muriate,) they will require 86 cubic inches, or three wine pint measures of carbonic acid gas. Or it will be equally convenient to immerse in the powder, diffused through a little water, a glass tube, discharging carbonic acid gas, from a flask containing chalk, and dilute sulphuric acid. If the bleaching-powder and water, be kept slightly warm in a little glass globe, into which the gas-tube is dipped, then the instant when the chloride is decomposed, and the lime carbonated, will be found by the liquid ceasing to make any permanent change on litmus paper. The muriate of lime is withdrawn by solution in water. The weight of calcareous carbonate must then be compared with that of the chlorine disengaged, from another equal portion of the powder, by dilute muriatic acid. Finally, the water may be found by distillation in a retort. Thus the analysis will be completed, with little manipulation and no fallacy *. For the purpose of commerce

* I have found on trial, the method by carbonic acid, to be exceedingly slow and unsatisfactory. After passing a current of this gas for a whole day through the chloride diffused in tepid water, I found the liquid still to possess the power of discharging the colour very readily from litmus paper. But the doctrine of equivalents furnishes a very elegant theorem with acetic acid, whose convenience and accuracy in application I have verified by experiment. An apparently complex, and very important problem, of practical chemistry is thus brought within the reach of the ordinary manufacturer. Since common fermented vinegar, is permitted by law to contain a portion of sulphuric acid, which avarice often leads the retailer to increase, we cannot employ it in the present research. But strong vinegar prepared from pyroligneous acid, such as that with which Messrs. Turnbull and Ramsay have long supplied the London market, being entirely free from sulphuric acid, is well adapted to our purpose. With such acid contained in a poised phial, fully saturate a given weight, (say 100 grains,) of the bleaching-powder, contained in a small glass matrass, applying a gentle heat at last, with inclination of the mouth of the vessel, to expel the adhering chlorine. Note the loss of weight due to the disengagement of the gas. (If carbonic acid be suspected to be

and the arts, however, the following very simple plan will very generally suffice. Get a glass tube, of about five cubic inches capacity, shaped as in the adjoining figure, and graduated into cubic inches and tenths. It is to be closed at top with a brass screw cap, and, at its recurved end below, with a good cork. Pour mercury into the upper orifice, till the tube be nearly full; leaving merely space to insert 10 grains of the bleaching-powder, made into a pellet-form, with a drop of water. Screw in the cap-plug, rendered air-tight by leather. Remove now the cork from the lower end, (also full of mercury,) and replace a little of the liquid metal, by dilute muriatic acid (sp. gr. 1.1). By dexterous inclination of the instrument, the acid is made to flow up through the mercury. Instantly on its coming into contact with the pellet, the chlorine is disengaged, the mercury flows out into a basin ready to receive it, while the resulting film of



present, the gas may be received over mercury, as formerly described.) Evaporate the solution, consisting of acetate and muriate of lime to dryness, by a regulated heat, and note the weight of the mixed saline mass. Then calcine this, at a very gentle red-heat, till the acetic acid be all decomposed. Note the loss of weight. We have now all the *data* requisite for determining the proportion of the constituents without solution, filtration or precipitation by re-agents.

Problem I.—To find the lime originally associated with the chlorine, or at least *not* combined with muriatic acid, and therefore converted into an acetate. *Rule.*—Subtract from the above loss of weight, its twenty-fifth part, the remainder is the quantity of lime taken up by the vinegar.

Problem II.—To find the quantity of muriate of lime in the bleaching-powder. *Rule.*—Multiply the above loss of weight by 1.7, the product is the quantity of carbonate of lime in the calcined powder, which being subtracted from the total weight of the residuum, the remainder is of course the muriate of lime. We know now, the proportion of chlorine, lime, and muriate of lime in 100 parts; the deficiency is the water existing in the bleaching-powder. Thus, for example, I found 100 grains of a commercial chloride sometime kept to give off 21 grains of chlorine, by solution in dilute acetic acid. The solution was evaporated to dryness; of saline matter

muriate of lime protects the surface of the metal, almost completely, from the gas. With an apparatus of this kind, (which indeed is the same as that which I have long used for analyzing limestones and marles, (see article *Carbonate*, in my Chemical Dictionary,) I get good accordances, with the results derived from the loss of weight, suffered by a like quantity of the chloride, when it is dissolved in dilute muriatic acid. Since a cubic inch of chlorine may be estimated in round numbers at $\frac{3}{4}$ of a grain, we may expect 10 grains of bleaching-powder to yield from 3 to 4 cubic inches of that gas; or by weight, from 20 to 30 *per cent.*, a wide range of power, which it is well worth the bleacher's or paper-maker's while to ascertain. If carbonic acid be suspected, we need only agitate the mercury through the gas, adding some of the metal, from time to time, as the absorption proceeds. The carbonic acid will remain, uncondensed at the top, and may be estimated in the usual way.

Sulphate of indigo, largely diluted with water, has been long used, for valuing the blanching power of chloride of lime; and it affords, no doubt, a good comparative test, though from the variableness of indigo it can form no absolute standard. Thus I have found, 3 parts of indigo from the East Indies, to saturate as much bleaching-powder, as 4 parts of good Spanish indigo.

M. Welter's method is the following:—He prepared a solution of indigo in sulphuric acid, which he diluted, so that the indigo formed $\frac{1}{1000}$ of the whole. He satisfied himself by experiments, that 14 litres (854.4 cubic inches, or 3.7 wine gallons, English,) of chlorine, which weigh $651\frac{1}{2}$ English grains, destroyed the colour of 164 litres of the above blue solution. He properly observes, that chlorine discolours more or less of the

125.6 grains were obtained, which, by calcination, became 84.3, having thus lost 41.3 grains. But $41.3 - \frac{41.3}{25} = 39.65 =$ lime present, uncombined with muriatic acid. And $41.3 \times 1.7 = 70.2 =$ the carbonate of lime, in the residuary 84.3 grains of calcined salts. Therefore, $84.3 - 70.2 = 14.1 =$ muriate of lime. Now, by dissolving out the muriate of lime, and evaporating, I got 14 grains of it, and the remaining carbonate was 70.3 grains. Hence this powder consisted of, chlorine, 21; lime, 39.65; muriate of lime, 14, and water $25.35 = 100$.

tincture, according to the manner of proceeding, that is, according as we pour the tincture on the aqueous chlorine, and as we operate at different times, with considerable intervals; if the aqueous chlorine or chloride solution be concentrated, we have the *minimum* of discoloration; if it be very weak, the *maximum*. He says, that solution of indigo, containing about $\frac{1}{1000}$ part, will give constant results to nearly $\frac{1}{40}$; and to greater nicety still, if we dilute the chlorine solution, so that it amounts to nearly one-half the volume of the tincture, which it can discolour; if we use the precaution to keep the solution of chlorine and the tincture in two separate vessels; and, finally, to pour both together into a third vessel. We should, at the same time, make a trial on another sample of chlorine whose strength is known, in order to judge accurately of the hue. On the whole, he considers that fourteen measures of gaseous chlorine, can discolour 164 measures of the above indigo solution, being a ratio of nearly one to twelve. The advantage of the very dilute tincture obviously consists in this, that the excess of water condenses the chlorine separated from combination by the sulphuric acid, and confines its whole efficacy to the liquor; whereas, from concentrated solutions, much of it escapes into the atmosphere. Though I have made very numerous experiments with the indigo test, yet I never could obtain such consistency of result as M. Welter describes; when the blue colour begins to fade, a greenish hue appears, which graduates into brownish-yellow by imperceptible shades. Hence an error of $\frac{1}{20}$ may readily be allowed, and even more, with ordinary observers.

It now remains merely to determine the average state, and equivalent weight, of the peroxide of manganese, so that the manufacturer of bleaching powder, may be able to mix his ingredients in due proportion. My first experiments on this subject were calculated to shew the quantity of chlorine, evolved from a certain weight of good commercial manganese, when acted on by liquid muriatic acid in excess. The chlorine gas was allowed to escape into the air, after passing through first a little water, and then a tube filled with drymuriate of lime.

I found that thirty grains of the manganese, thus treated, occasioned to the apparatus, a loss of weight in one experiment of 17.8 grains chlorine, and in another of 17.5 grains. Hence 100 grains were equivalent to about 59 of chlorine. I found afterwards, by an imperfect analysis, that 100 grains of the same manganese contained 10 of siliceous matter, 4 of water, and a very little iron, probably, in all, about 16 grains, leaving 84 of oxide of manganese. Thus apparently 100 grains of pure peroxide of manganese should yield 70 of chlorine with muriatic acid; but we shall presently see, by other and more rigid methods, that 100 grains are equivalent to no less than 81.80 of chlorine. Is this difference to be ascribed to the native oxide of manganese, containing a portion of protoxide, or to some other contamination of which I took no account? I shall endeavour to answer this question by future researches; be this as it may, we obtained above 59 grains of chlorine, whose volume is 77⅓ cubic inches. Hence, 100 grains of such manganese yielded 19.526 grain measures. M. Welter says, that it required 61 grammes of his oxide of manganese to develope 14 litres, or 14,000 gramme measures of chlorine, which gives the proportion of 100 to 22.900. His manganese must have been at this rate much better than mine. The pure peroxide yields from 100 grains, 27.090 grain measures.

From both the sulphate and muriate of manganese, purified in Mr. Hatchett's elegant method, I procured proto-carbonate, by adding solution of crystals of soda. This carbonate was well washed and dried in a vapour, bath at 190° F. The proportion of carbonic acid which this salt contained was determined by the loss of weight it suffered by solution in dilute sulphuric acid, and also by collecting over mercury, the volume of acid gas disengaged from it by a red heat. It was thus found that 100 grains contained 35.4 of carbonic acid. From another, 100 grains of the carbonate, I obtained, by distillation in a glass retort, 7.3 grains of water, leaving of dry proto-carbonate 92.7 grains, which contained 35.4 carbonic acid, and 57.3 of protoxide of manganese; but 35.4 : 57.3 : : 2.75 : 4.42, a number, therefore, representing the prime equivalent of protoxide

of manganese to that of carbonic acid, 2.75. We shall presently see, however, by another experiment, on Richter's principle of reciprocal saline saturation, lately revived by Doctor Thomson, that 4.5 is the true number, in exact accordance with this chemist's late atomical determination *. Hence, instead of 35.4 grains of carbonic acid, 35.2 should have been obtained; an error within the limit of careful experimenting. The proto-carbonate, formed as above, consists, therefore,

of carbonic acid . . .	2.75	35.2
Protoxide of manganese	4.50	57.6
Water, 1 atom to 2 atoms carbonate . . .		7.2
		100.0

Sulphate of manganese dried at a heat of 212° F., consists of one atom of water and one atom of sulphate of manganese. Of the same salt, exposed to very gentle ignition, I took 9½ grains (= 5 acid + 4.5 oxide), and dissolving them in water, added to this solution, another containing 13.25 of muriate of barytes (= 45 chlorine + 8.75 barium). After subsidence of the sulphate of barytes, the supernatant muriate of manganese, was found to be entirely free from every trace of both sulphuric acid and barytes. Therefore, 5 sulphuric acid are equivalent to 4.5 protoxide of manganese; and, of course, 2.75 carbonic acid to the same weight of oxide.

Thirty grains of proto-carbonate, containing, as above, 17.31 of protoxide, formed by ignition in a platina capsule 21 of black peroxide. But 17.31 : 4.5 : : 21 : 5.46, a number which clearly indicates 5.5 to be the atomic weight of this substance, which is therefore a deutoxide. The prime equivalent of the metal is consequently 3.5; the intermediate oxide of which Dr. Forschammer treats, appears to me to be the result of a combination of the above two, or it holds the same relation to them that minium does to lithage, and to the puce-coloured oxide of lead.

We may now conclude that 5.5 parts of pure deutoxide of manganese, when acted on by muriatic acid, should afford 4.5

* *Annals of Phil.*, *New Series I.* p. 241.

parts of chlorine gas; or 100 grains of the former should yield 81.8 grains of the latter, equivalent in volume to 107.28 cubic inches. To afford this quantity of chlorine, two atoms of muriatic acid must act on one atom of peroxide; one atom of hydrogen quits the muriatic acid, to unite with one atom of oxygen in the oxide, forming an atom of water; the corresponding atom of chlorine is disengaged; while the resulting atom of metallic protoxide attracts and neutralizes the second atom of muriatic acid. Or in numbers 5.5 parts of peroxide of manganese, take $4.625 \times 2 = 9.250$ of muriatic acid gas, which by the above table* are present in 100 grains of liquid acid, specific gravity 1.045. Thus 10 grains peroxide of manganese require 16.8 of muriatic acid gas, equivalent to 100 grains of liquid acid of 1.082.

When a mixture of sulphuric acid, common salt, and black oxide of manganese are the ingredients used, as by the manufacturer of bleaching powder, the absolute proportions are:

1 atom muriate of soda .	7.5	29.70	100.0
1 atom peroxide of manganese	5.5	21.78	73.3
2 atoms oil of vitriol 1.846	12.25	48.52	163.3
	25.25	100.00	

And the products ought to be;

Chlorine disengaged, . .	1 atom	4.5	17.82
Sulphate of soda	1	9.0	35.64
Proto-sulphate of manganese	1	9.5	37.62
Water	2	2.25	8.92
		25.25	100.00

These proportions are, however, very different from those employed by many, nay, I believe, by all manufacturers; and they ought to be so, on account of the impurity of their oxide of manganese. Yet making allowance for this, I am afraid that many of them commit great errors in the relative quantities of their materials. Thus I have been informed by one respectable manufacturer, that he employs 10 of salt, 12 of sulphu-

* See last Number of the Journal, p. 289.

ric acid, sp. gr. 1.846 (or rather its equivalent of dilute acid, 1.65), and 14 of manganese. Others employ a great deal less manganese; about 70 or 80 to 100 of salt. It is easy, however, to get good approximate proportions for practice. My experiments on the quantity of chlorine, producible from a certain weight of good English manganese, gave 59 grains of the former for 100 of the latter. But 59 parts of chlorine are equivalent to $98\frac{1}{3}$ of common salt; hence in round numbers 100 parts of such manganese would have required 100 parts of common salt. Therefore the true proportions become 100 of common salt, 100 of manganese; $81\frac{2}{3}$ oil of vitriol for saturating the soda in the common salt, and $95\frac{3}{4}$ oil of vitriol for saturating the 86 of metallic oxide in 100 of manganese ore; constituting of the liquid acid 177.4 parts, to 100 of each of the other articles. As oxide of iron, so often mixed with manganese, has nearly the same prime equivalent, we reckon its saturating power the same in the above calculation. To add a quantity of salt beyond what is equivalent to the manganese and oil of vitriol is impolitic; it occupies the alembic unnecessarily, and obstructs the mutual action of the muriatic acid and manganese. And, if the oil of vitriol be not adequate to separate from the salt, as much muriatic acid as the manganese present can change into chlorine, the process is injudicious and wasteful. The approximate value of manganese for this manufacture may be readily ascertained by the recurved glass tube, described above, for the analysis of bleaching powder. The film soon formed of dense muriate of manganese, protects the mercury. Or the chlorine disengaged from 100 grains of manganese covered with muriatic acid in a matrass, may be conducted by a glass tube into a dilute solution of a known sulphate of indigo.

From the preceding computation, it is evident that 1 ton of salt with 1 ton of the above native oxide of manganese properly treated, would yield 0.59 of a ton of chlorine, which would impregnate 1.41 tons of slaked lime, producing 2 tons of bleaching powder, stronger than the average of the commercial specimens; or allowing for a little loss, which is

unavoidable, would afford 2 tons of ordinary powder, with a little more slaked lime.

Since writing the preceding paper, I have prepared a carbonate of manganese, by adding carbonate of soda to the metallic sulphate, and after thorough edulcoration, dried the precipitate in an exhausted receiver, containing a basin of sulphuric acid. Its constitution is exactly 2 atoms of carbonate of manganese + 1 atom of water, as above given.

In conclusion, I may state that some manufacturers have generated this chlorine for making oxymuriate of lime, by passing a stream of muriatic acid gas, evolved from salt and oil of vitriol, over a body of manganese. Thus the resulting sulphate of soda is not contaminated with sulphate of manganese and iron, as in the common method.

Glasgow, December 7, 1821.

Art. II. *On the Neglect of the Bath Waters in the Cure of Disease.*

[In a letter to the Editor of the *Quarterly Journal.*]

Sir,

What are the causes of the present neglect of Bath waters in the cure of disease? is a question so frequently asked, that I have judged it my duty, as a physician actually practising here, to consider the subject with all the attention it is in my power to bestow. The result I beg to lay before the public, through the medium of your Journal.

The external use of these waters is very ancient, so ancient indeed as to be lost in fable. Their use internally is said to have commenced in the reign of Charles II., or toward the end of the seventeenth century; and it seems to have increased progressively, until within the last twenty or thirty years. During this period, it is notorious that they have gradually

sunk in reputation; how undeservedly, will appear in the sequel. Had they not been possessed of active properties, but, like many fountains that might be named, derived a spurious character from some saint or legend, it might be imagined that a superstitious belief in their efficacy had yielded to the general spread of knowledge in this enlightened age. But even those who mainly contribute towards their decline, have never presumed to tax them with inefficiency; and it will not be difficult to shew that if, in any instances, they have proved hurtful, they are infinitely less so than the remedies which their opposers would fain have preferred to them.

Without further preamble, then, I shall proceed to answer the question proposed. In the first place, it appears to be the effect of one of those revolutions which are continually happening in the moral, as well as in the natural, world; and from that view of the subject we shall derive satisfaction, for it would be as absurd now to suppose that these salutary springs will not recover their celebrity, as it was a few years since to dread an approaching assimilation of our climate to that of Spitzbergen or Nova Zembla, because a few successive summers had proved below the average temperature. Let it not, however, be conceived that I am so sanguine as to expect they will as suddenly regain their character, as the climate has through the occurrence of the hot and dry summer of 1818. In the moral world the progress of change is somewhat more slow, but scarcely less certain than in the natural; and therefore I feel persuaded that, although we may not "return to nature" with Mr. Newton and Sir Richard Phillips, we shall certainly, one day or other, return to the use of the Bath waters with common sense and experience for our guides.

It is not enough, however, to have referred these phenomena to certain revolutions. It will be necessary to inquire by what agency they have been produced. As it is not my object, however, to compose a treatise upon climate, I shall confine my remarks to the immediate agents through which the moral, or rather medical, change under consideration appears to have been effected.

These are of two kinds, domestic and foreign. The domestic are certain speculative views in medical doctrine, which have prevailed in this city during the above-mentioned period. I need not add that the practice has corresponded with the theory. They originated with a gentleman of commanding talent and persuasive manner, who, for many years, enjoyed the highest reputation, having scarcely any competitor, and being singularly felicitous in the ready adoption of his opinions by the subordinate members of the profession; who, from his general and extensive practice, and freely communicative disposition, could not fail to become acquainted with them. These views, it is well known, were altogether unfavourable to the use of the Bath waters, for if diseases originate " either in absolute or relative excess of momentum, impetus, or determination of blood, in some portion of the arterial system of the part affected *, (and I do not know that there were any exceptions to this doctrine,) it was not very likely that the Bath waters, which have tonic and stimulant properties, though of a mild kind, should have at any time been considered an appropriate remedy. So generally have the medical practitioners of this place been thus influenced in their practice, that a great proportion of persons sent hither to drink the waters, have not been allowed to taste it. Some, however, in opposition to their opinions, have preferred the advice which sent them here, and taken the forbidden drink, with a success that has falsified the fears of those gentlemen, and excited the ridicule of the patients by whom they have been thus exposed. But a few instances of this kind were not sufficient to correct the general practice, which has proved highly injurious, not only to the reputation of the waters, but likewise to themselves; for those distant practitioners, whose patients, instead of drinking the waters, were subjected to plans of treatment which they never contemplated, have been so disgusted, as to lose all confidence in a remedy undeservedly subject to such frequent prohibition, and have therefore discontinued their recommendation of it.

* See *Elements of Pathology and Therapeutics*, by Caleb Hillier Parry, M.D. F.R.S., vol. i. p. 358.

The foreign causes of the neglect of Bath waters, in the cure of disease, are the writings and opinions of physicians, and other practitioners, chiefly resident in the metropolis, amongst whom the late Dr. Saunders holds a conspicuous place. This author, during the period already mentioned, published a book upon mineral waters, in which he pronounced judgment upon those of Bath, without ever having witnessed their exhibition upon the spot, where only evidence of their powers can be obtained; for when conveyed to a distance, their most active properties suffer diminution, if they be not entirely dissipated. In what he says of the medical powers of the waters, he admits that they possess considerable efficacy in some cases, when administered internally; but he lays so much stress upon the hazard attending their exhibition, that many have been discouraged in their hopes of relief, who might otherwise have derived the greatest benefit from them. To the external use of them he denies any superiority above common warm water. It is scarcely necessary to say that this opinion is wholly disproved by the records of the Bath Hospital.

But it is not in respect to its warm springs only that the city of Bath suffers from the presumption and vanity of this author. In describing its site, he says, " the older part of the city is contiguous to the Avon, is narrow, irregular, and mostly ill-built, and has its foundation on a marshy and clayey soil." Now the first part of the description is all that it contains of truth. The older part of the city is certainly contiguous to the Avon, from which circumstance great advantages are derived in regard to cleanliness. He might have added that much of the new part is similarly situated, but the streets, in both parts, are neither narrow, irregular, nor mostly ill-built, as every person who has visited Bath can testify. The most erroneous part, however, of the description is that which relates to the foundation, which he says is on a marshy and clayey soil. So far is this from being true, that the soil upon which the lower part of the city stands, consists chiefly of alluvial gravel, the *detritus* of the neighbouring hills, than which nothing can afford a more dry or healthful foundation.

He states, moreover, that Bath is subject to the inconvenience of a larger proportion of rain than falls on the eastern side of the kingdom. This is certainly true, but it is not the whole truth, and it leads readers to consider the situation as worse in this respect than it really is. The average fall of rain here during seven years was 27 inches, which is only three inches more than at Tottenham, and is far below the average of the whole kingdom. If the average fall here be compared with that at Gosport, (30 inches,) or at the Isle of Wight, (36 inches,) Bath cannot be regarded as a rainy situation.

So much for the information to be derived from this popular work. Its logic is even more extraordinary, for the author endeavours to prove that a looker-on, or a person who sees what happens in a place from which he is more than one hundred miles distant, "has superior advantages in forming a general and comparative estimate of matters so much in detail as the medical powers of mineral springs, to one whose labours in this undertaking have been aided by industry, scientific knowledge, and opportunities afforded by a residence on the spot." Preface, p. 11.

I have stated the foreign causes of the neglect of Bath waters in the cure of disease to be the writings and opinions of physicians and other practitioners, chiefly resident in the metropolis; and having noticed κατ᾽ἐξοχὴν the work of Dr. Saunders, it remains for me to observe that his opinions have been adopted by some physicians, though, I believe, not by many. As to the other practitioners, it will be observed that I have not styled them medical, and this may at first excite surprise; for few persons would suppose that the writings and opinions of the surgical part of the profession, could affect a circumstance so entirely out of their province. Yet such is actually the case. I do not mean to say that any of them have composed treatises upon the internal use of the Bath waters, or that they have promulgated opinions which have had a direct tendency to injure their reputation with the public. This has, however, been done, indirectly, by one of this class of practitioners; who scorning the limits of a laudatory but arduous contention with his

competitors for surgical pre-eminence, has stepped forward as the rival of the whole college of physicians, and put forth a work upon constitutional diseases, of so comprehensive a nature, and such confident pretensions, as to attract more than ordinary notice. According to the new light which has thus broke in upon us, the endless variety of diseases, with all their complicated phenomena, are resolved into merely sympathetic affections, connected with a deranged state of the organs of digestion, which a few doses of blue-pill, with or without other gentle purgative medicines, will speedily remove. The simplicity of this doctrine has secured its ready adoption by the author's pupils, it having been propounded by him in his lectures for many years past to a numerous class, consisting chiefly of young men from the country, who, having every thing to learn in six months, are pleased to find so easy a road to medical practice and medical fame marked out for them by their learned teacher. Accordingly, imbued with principles thus dexterously acquired, they have entered upon the practice of their profession, and introduced the whole of his notions about the digestive organs, together with his concise method of curing disease, into every part of the kingdom *. Many proselytes, likewise, have been made by the book, amongst those persons who could not have an opportunity of hearing the lectures upon this discovery. Through the numbers, then, who have either drank at the fountain-head of MEDICAL SURGERY, (for

* In general, I believe, they have implicitly adhered to it, but in this place a monstrous extension of it has been made, by one individual, to a part more properly termed *dejective* than *digestive*; and, with so much success that a great many of our fashionable visiters, who might have drank the Bath waters, and in all probability received a cure from them, have been induced to submit, not merely to repeated deglutitions of blue-pill, but likewise, *horresco referens!* to the most disgusting and dangerous treatment of a mechanical kind. In some instances, too, this has been accompanied with an appearance of satisfaction, not unlike that of the courtiers of Louis XIV., who, by a strange perversion of sentiment and loyalty, were not only proud to be the favourites of the Grand Monarque, but of being likewise counted martyrs to the same disease with which that personage is known to have been afflicted.

this is the name of the new school,) or who have partaken of its streams in the work alluded to, the practice there recommended is now a very prevailing one throughout great part of the British Isles.

A doctrine so simple and attractive, and a practice so convenient in respect to time and place, cannot fail to prevent invalids from resorting to mineral springs of a tonic nature, and must therefore be enumerated amongst the causes of their present neglect in the treatment of diseases. But there is a fashion in all these matters, and though the dominion of blue-pill, is so powerful and pervading now, it cannot be enduring. They who have seen the downfall of others, not less powerful in their day, perceive that it totters already, and smile at the folly of those who have successively imagined that a bottle of brandy, a phial of laudanum, a lancet, or a box of blue pills, is sufficient for the cure of the manifold diseases that afflict the human race.

Lest your readers should suppose, from the circumstance of my being a resident physician in Bath, that I am attempting to write up the waters, by writing down blue-pill or any other remedies, I beg to deprecate such a construction upon my endeavours. My sole object is to restore its wonted character to a neglected remedy of great value, by shewing that, during the last twenty or thirty years, certain speculative views of disease, without any thing save their speciousness to recommend them, have led to the indiscriminate adoption of plans of treatment, in which the Bath waters, even as auxiliaries, have been quite overlooked. I am as little disposed to think that the remedy whose cause I am advocating will cure all diseases, as I am that blue-pill, or the lancet, possess such "charmed" virtue. But at some future time I may probably undertake to prove that it is capable, when judiciously administered, of effecting much good, with infinitely less harm, than either of those much-boasted means.

I would rather caution them against placing implicit belief in those persons who employ, in every disease, any single remedy,

or any particular mode of treatment, to the exclusion of all others, an error into which the more superficial professors of the healing art have at all times fallen. Accordingly, Gideon Harvey, in his very severe satire upon those of the seventeenth century, has divided them into the following sects, according to their peculiar and exclusive modes of treating diseases. 1. Orareo-ferreo-medici, who cured all their patients with preparations of iron. 2. Asinarii Doctores. The patients of these asinine doctors were indiscriminately ordered to drink asses' milk. 3. Jesuitici Doctores, who were equally addicted to Peruvian bark; and he wittily adds, *à capite ad calcem defraudantes.* 4. Aquarii-medici, whose patients were all sent to drink mineral waters. 5. Lanii Doctores. These had recourse to the lancet in every disease. 6. Stercorarii Doctores. These were quite as positive in maintaining that all diseases were to be expelled by means of purging medicines. After which classification he emphatically says, "*Hic autem ars medica ad instar monstri sex pedibus incedere videtur.*" Had he, however, lived in these days, he must have given the beast another foot, to designate a sect of which he had no conception, namely, Hydrargyri Doctores, who attack all diseases with one and the same mercurial weapon. Or he might rather have regarded it as a monster with only one foot, of a blue colour; a true Gasteropode.

A physician of great respectability, retired from practice, had the curiosity, during the last autumn, to look over a file of prescriptions in a chemist's shop. He examined thirty, of which number twenty-eight consisted of blue pill!

Can such things be,
And overcome us like a summer's cloud,
Without our special wonder?

I have the honour to be, Sir, &c.

A Resident Physician in Bath.

To W. T. Brande, Esq.

Art. III. *An Account of the Periodical Literary Journals which were published in Great Britain and Ireland, from the year* 1681 *to the commencement of the Monthly Review, in the year* 1749. *By* Samuel Parkes, F.L.S., M.R.I., &c.

The French "Journal des Scavans," and the "Republique des Lettres," printed at Amsterdam, are well known to the learned; but at this day few persons are acquainted with the extent of the efforts made in Great Britain, in the latter part of the seventeenth, and the beginning of the eighteenth, century, to communicate to the public, by means of periodical works, interesting intelligence respecting the nature and design of the various publications which were then issuing from the British press. And as so long a time has elapsed since the first literary journal was published in Great Britain, I conceive no apology can now be necessary, for laying before the English reader a consecutive account of such publications, from their commencement to the middle of the last century and this I shall attempt with as much brevity as is consistent with the object I have in view, after having said a word or two respecting the foreign journals above mentioned.

The "Journal des Scavans," which commenced in 1665, was planned by M. Denis De Sallo, who was an eminent Greek and Latin scholar, and an authorized ecclesiastical counsellor in the parliament of Paris. According to Voltaire, this accomplished man was the *inventor* of literary journals, and although the "Journal des Scavans" was the first work of the kind, it very soon acquired so much reputation that it was circulated and read in every country of Europe. M. De Sallo, however, published only the three first journals, and these were printed under the name of *Herouville*, which was that of his valet-de-chambre. He then resigned the work to the Abbe Gallois, who, patronised by the great Colbert, continued the publication until the year 1674, when he was obliged to relinquish it, in consequence of his having been chosen librarian to the French king, and professor of Greek in the royal college.

The Abbé Gallois was succeeded in the undertaking by Mons. De La Roque, who continued to conduct the work until the year 1683, when he was succeeded by Mons. Cousin, who was editor until the year 1702. After this period the plan of the journal was much improved, and then was published under a new form, making from the first more than 350 volumes. The work was for some time discontinued, and has lately been revived at Paris, in quarto.

The "Republique des Lettres," which commenced in 1684, was planned and edited by the celebrated Peter Bayle. It was published monthly at Amsterdam, in small 24mo, under the title of "Nouvelles de la Republique des Lettres," and formed two volumes a year. It was continued until the year 1689 to the end of the eleventh volume, when Mr. Bayle, in consequence of illness, gave it up to his friends M. Bernard and M. De la Roque, who continued jointly to superintend the publication until the year 1699. After an interval of nine years the work was resumed by M. Bernard, and continued by him until the year 1710.

The only attempts that have been made to form a correct list of the British literary journals, that I know of, are by Mr. Dibdin, in his "Bibliomania," page 20; and by Mr. John Nichols, in his "Literary Anecdotes of the Eighteenth Century." In the former of these lists some of the most voluminous and important of the early journals are entirely omitted, and in that by Mr. Nichols, which he says he copied from Asperne's account of the late Mr. Griffiths, there are many omissions, and some errors of considerable importance, especially as to the date of the first English work that appeared of the kind now under consideration. Thus, the "Weekly Memorials, or an account of books lately set forth, with other accounts relating to learning; by authority," (the first number of which was published on the 19th January, 1688-9,) Mr. Nichols says, is the earliest specimen of an English review,

whereas the book which I have to quote below, with a title somwhat similar, professes to be an account of books for the years 1681, 1682, and 1683; and this was known to Mr. Dibdin, who mentions it in his list, and says it was "the earliest publication in this country in the character of a review."

It is now many years since I first met with a few volumes of these early productions, which I found to be so interesting and instructive, that I have been endeavouring to complete my series of them ever since. As books of reference I have found them extremely useful, and so much so, that I have no hesitation in saying that, as far as they can be procured, they ought to find a place in every good English library.

The following is the most complete list that I have been able to make out of these early literary journals, all of which, with a few exceptions, are in my own possession.

The titles of the respective works, which were published in the following order, are expressed thus:

I. * 1681. "Weekly Memorials for the Ingenious: or an account of books lately set forth, in several languages, with some other curious novelties relating to arts and sciences." Quarto, London, printed for Henry Faithorne and John Kersey, and sold at their shop, at the Rose in Saint Paul's Churchyard.

The volume, which is dedicated to the Honourable Robert Boyle, contains fifty weekly numbers; the first is dated Monday, January 16th, 168½, the last number bears the date of January 15th, 1683. From the preface I have made the following extracts: "I cannot doubt but this weekly paper will find a candid and cheerful acceptance amongst all ingenious persons, the design of it being to present them with a short view of the worthy labours *daily* set forth by the learned. The bare titles of books yearly printed in our common catalogues are somewhat dry things, scarce able to raise in men that gust and

* It must be recollected that these numbers are entirely arbitrary, being inserted merely for the convenience of reference.

appetite to learning, which we may hope these brief accounts will give them. I shall not confine myself onely to authors of our own nation, but shall likewise give accounts of most books transmitted to us from all other parts, and shall transcribe from the PARIS JOURNAL DES SCAVANS the curious novelties therein contained. Mr. Oldenburgh in his 'Philosophical Transactions, commonly gave us something from the French journals, Mr. Hooke, in the first of his two Philosophical Collections, having done the like. But by reason of Mr. Hooke's other considerable employs, these have been long intermitted, which has made me enter into thoughts of giving accounts from them. If the Royal Society shall think my endeavours in this kind any way subservient to their designs, it may animate my industry to perform things in the best manner I may, none being more devotedly their servant than myself." The editor of this work was the ingenious and indefatigable James Petiver, and I doubt not but he was the author of the preface which I extracted. But more of this great man hereafter.

The volume is very neatly printed, and contains some good copper-plate engravings of subjects in natural history, with some other curious designs on the letter-press. The last paper but one in the volume is entitled, "Historia Moschi ad Norman Academiæ Naturæ Curiosorum, Conscripta à Luca Schrockio, M.D., 1682, quarto," and it is embellished with a very neat copper-plate engraving of the musk animal, with two other descriptive drawings, one of the musk-bag, with the long fine hairs that surround it; the other, shewing the internal structure and texture of the bag.

I perceive, however, that some of the most valuable articles in the volume are English translations from the French "Journal des Scavans."

II. 1687. "THE UNIVERSAL HISTORICAL BIBLIOTHEQUE, or an account of most of the considerable books printed in all languages, in the month of January, 1686, wherein a short description is given of the design and scope of almost every

book, and of the quality of the author, if known." London, quarto, printed for Geo. Wells, 1687.

This important work was planned and published by the learned John Le Clerc, a native of Geneva, who early in life settled in London, but soon removed on account of his state of health to Amsterdam, where he published the greater part of his works, English as well as French. The first number of the "Bibliotheque" for Jan. 1686, was printed in 1687, and it was continued by him, with the assistance of Mr. De la Crosse, to the end of the eleventh volume; the subsequent volumes, to the nineteenth, were written solely by Le Clerc, and the last six volumes by his relative, the learned Mr. James Bernard. The whole work consists of twenty-five volumes in quarto, and was completed at the close of the year 1693.

In 1688, Cave first published his "Historia Literaria," in two volumes folio, which is a review of ecclesiastical writers, and is considered to be a work of great research, but as it was not published periodically, it does not come within my plan.

III. 1690. "Censura Celebrium Authorum;" by Sir Thomas Pope Blount, bart., in one volume folio. The author of this work was a man of great erudition, and his book has always been much esteemed by the curious. In this review of the writings of a variety of eminent men of his own and former times, he reports their opinions in their own language, which renders the book of more value than it would otherwise have been. The "Censura" was not published periodically, but appeared at first in the year 1690, in one volume, folio, and was printed in London. It was reprinted at Geneva, in quarto, in 1694, and again in 1710 and 1718. This learned man published also a work in quarto, entitled, "Remarks on Poetry," London, 1694, which is a kind of Censura, confined to an examination of the merits of the poets. Sir Thomas was also the author of a volume of essays on a variety of subjects, printed in octavo, in 1697. Respecting these essays, Mr. Chalmers says, that, "in point of learning, judgment, and freedom of thought, they are no way inferior to those of the celebrated Montaigne."

IV. 1691. "THE ATHENIAN MERCURY, resolving weekly all the most nice and curious questions proposed by the ingenious." Folio, London, printed for John Dunton, at the Raven, in the Poultry, 1691. In an advertisement the editor engaged to give an account of all new books, in a supplement to each volume. The work was published every Tuesday and Saturday, a single leaf at a time, closely printed on both sides *, *price one penny*. It commenced on the 17th March, 1691, and was regularly printed twice a week, until the 8th February, 1696, forming nineteen very thin folio volumes of thirty numbers, or sixty pages each. It was then discontinued until the 14th of May, 1697, when the first sheet of the twentieth volume was published, but how much more was printed I have not been able to learn. A supplement to each volume was also published about once in every three months. The title of one of these is thus expressed, "The supplement to the third volume of the "Athenian Gazette," containing an account of the design and scope of the most considerable books newly printed in England, and in the foreign journals, and of the quality of the authors, if known. With impartial remarks upon their undertaking and performance. These supplements will be continued constantly, by several learned persons, and comprehend a brief idea of all valuable books published from time to time."

* At this time, and for twenty years before and afterwards, it was no uncommon thing for books to be published at stated periods, and one leaf only at a time. Sir Roger L'Estrange published a folio work in this way, called the "Observator," the first number of which appeared on 13th April, 1681. The following are a few scarce volumes which came out in the same way, *viz.*, the "Observator," published every Wednesday, at 1*d.* each, commenced April 1st, 1702, The "Examiner," every Thursday, in folio, commenced Aug. 3d, 1710. The "Whig Examiner," also every Thursday, commenced Sept. 14th, 1710. The "Hermit," every Saturday, at 1*d.* each, commenced August 4th, 1711. These were all on literary subjects. Besides these, I have seen a large folio volume, entitled, "Mercator, or Commerce Retrieved," which came out every Tuesday and Saturday, in single leaves, price 1½*d.* each, the first number of which made its appearance on the 26th May 1713.

To the last number of the nineteenth volume, the following advertisement was annexed: "This is to give notice, that the proprietor thinks fit, whilst the coffee-houses have the votes every day, and six newspapers every week, to discontinue this weekly paper, (the 19th volume being now finisht,) and carry on the design in future in volumes; and, in pursuance of this resolution, 30 numbers shall speedily be printed all together, to compleat the 20th volume, and after that an intire volume shall be publisht quarterly. This is further to give notice, that besides the quarterly publication, the first undertaker likewise designs to have it continued again in WEEKLY PAPERS, as soon as ever the GLUT of NEWS is a little over."

The whole of this very curious work is comprised in about 1500 pages, and is usually bound in two or three volumes in folio. My own copy extends to No. 10, of the 20th volume, printed June 14th, 1697.

It seems that the Marquis of Halifax, Sir William Temple, and the learned Sir Thomas Pope Blount, very much patronised this work, and that Sir Wm. Temple was a frequent contributor to it. We are told by John Dunton, that "these volumes growing quite out of print, a choice collection from them, in three volumes, have lately been reprinted and made public, under the title of the 'Athenian Oracle.'" He adds, "the copy of these three volumes I sold to Mr. Bell, in Cornhill, and it is thought he will get above a thousand pounds by it*." A pretty good sum in those days for the copyright of three octavo volumes.

V. 1691. "THE HISTORY OF LEARNING, or an abstract of several books lately published, as well abroad as at home." By one of the two authors of the "Universal Historical Bibliotheque." Quarto, London, printed for Abel Swalle and Timothy Childe, at the Unicorn, 1691.

The first number of this work was dedicated to the Earl of Dorset and Middlesex, Lord Chamberlain of their Majesties houshold, and signed J. D. de La Crose. Then follows a pre-

* See Dunton's "Life and Errors." London, 1705, page 262.

face, from which I shall make a few extracts, to shew the nature and plan of the undertaking.

"I design," says the editor, "to publish an abridgment of all new books as they shall appear in the world; to which purpose I shall keep a correspondence abroad, in order to the being furnished with every thing rare at the first. But in regard this design is of too large extent, that is, the abridging of every book that is published, especially in this age, where so many trifling impertinencies pass the press, I shall chuse only such to insert in this work as may most deserve the perusal of the studious reader." Having given a list of the kind of books that he did not intend to review, he adds, "but I would not be thought, upon this pretence, to excuse myself from abstracting from ingenious treatises in anatomy, natural philosophy, and mathematicks, for though such abstracts may be unpleasant to such as understand them not, *they* must bear with the evil, and remedy it by turning over the leaf, to a place that pleases them better." "In cases where a person may have made any discovery in natural philosophy, physick, mathematicks, critick, or the like, and would not give themselves the trouble of writing a treatise upon it, if they please to communicate it to us, we shall give it a place in our journal, and preserve and publish it to the world, better by far than if it was printed by itself. Which advertisement, considering the present discontinuance of the 'Philosophical Transactions,' will not, we hope, seem impertinent to the learned world."

I suspect, however, that only a few numbers of this work were published, because in the month of August, in the same year, we find Mr. De la Crose had formed a connexion with other booksellers, and began a similar work under a different title, as follows:

VI. 1691. "The Works of the Learned, or an historical account and impartial judgment of books newly printed, both foreign and domestick, to be published monthly." By J. De la Crose, a late author of the "Universal Historical

Bibliotheque. Quarto, London, printed for Thomas Bennet, at the Half Moon, St. Paul's Churchyard.

The first number of this work made its appearance in August, 1691, with a Latin dedication to Henry Compton, Lord Bishop of London. A singular address to the reader is also prefixed, in which, among other things, he says, "If you judge of this by the 'History of Learning,' printed with my name, you will have perhaps a mean opinion of it; but I was unluckily ingaged with a bookseller who thought himself wiser than I, and would overrule in every thing. As to this, things are now changed, and you may be sure, (provided God preserves us in health,) that during a year, the first MONDAY in every month, you shall have a small book like this, and better I hope, if learned men approve of my endeavours, and vouchsafe to send me their observations, and direct them to the bookseller. If some authors ask by what right I take upon me to pass a judgment over their books, I answer, by the same right as they have published them. Every one may take the same liberty with me: if they give me wholsom advice they shall be thanked for it, and I 'll mend the faults as well as possible. Otherwise I don't love wrangling, and have not much time to lose; and though scribblers should write every day against me, I shall not trouble the world with a word of answer. I have now to say to those that shall have the civility to send me their books, that I shall keep a note of the time wherein I have received them, and put them in my journal according to the date of their reception. But that they may not lose their time *and their books*, they must remember that this is an account of the WORKS OF THE LEARNED."

The first volume of this undertaking, which commenced in August 1691, was completed in April 1692, and was comprised in 418 quarto pages, containing a great deal of very curious matter, closely printed, with marginal notes. The last number of the volume, however, contains the following advertisement from the bookseller. "A monthly Journal," says he, "returning too quick, to have it always filled with considerable

books, from this time I shall print *the works of the learned* only four or five times in the year, and none but books of note shall make any part of it." The work with this title, by Mons. de la Carose, is therefore complete in one volume of 398 pages, to which are appended two good indexes, one of the titles of the books reviewed, the other a table of the several matters contained in the volume.

VII. 1692. The Young Student's Library, containing Extracts and Abridgments of the most valuable Books printed in England, and in the foreign journals, from the year 1665 to this time: to which is added, a New Essay upon all sorts of Learning, wherein the use of the sciences is distinctly treated on." By the Athenian Society. Folio, London, printed for John Dunton, at the Raven in the Poultry, 1692. This volume, which has a very fantastical copperplate engraving in folio, representing the Athenian Society, contains a very elaborate review of the following publications, among others of lesser note. The works of Dr. Lightfoot, of Bishop Usher, and of Dr. Barrow: the Letters of Grotius, Sir John Chardin's Travels in Persia, the works of Boyle, Locke's Essay on the Human Understanding, &c. &c., with ample extracts from each. It also contains observations on the Philosophical Transactions, and copious extracts from the foreign philosophical journals.

The Young Student's Library does not appear to have been published in parts, but as it consists entirely of a review of the most valuable books printed at that time, and as it laid the foundation for the following periodical work, it was thought to deserve a place in this list.

VIII. 1692. "The Complete Library; or, News for the Ingenious. Containing several original pieces; an historical account of the choicest books newly printed in England, and in the foreign journals; as also the state of learning in the world." To be published monthly. By *a London Divine*, &c.

Quarto, London, printed for John Dunton, at the Raven in the Poultry. 1692.

This work was advertised as a continuation of the "Young Student's Library," and was published by the same bookseller, monthly. The number for December 1693 closes the second volume. Four numbers of a third volume were also published, and then appeared the following advertisement. "Many gentlemen that were wont constantly to take in the *Complete Library*, being gone into the country, it is designed that the Complete Library for *May*, *June*, *July*, *August*, *September*, *October*, *November*, and *December*, shall be published all together at the end of the year, which shall contain all the valuable books published from time to time, and also the state of learning for every month, which will complete this third volume." These numbers, I suspect, were, however, not published, as the following statement appeared in the Athenian Mercury for December 1, 1694. "There is now published weekly MISCELLANEOUS LETTERS, giving an account of the works of the learned; upon which account the author of the *Complete Library* has wholly discontinued his monthly journal." My copy of the Complete Library goes no farther than April 1694, and I believe no more numbers were printed. The two first volumes of this valuable work are said to be written by a "London divine," but the title-page of the third volume purports to be by R. W., M.A. I have reason to think that this early journal is now very rarely to be met with; I never saw any except my own copy; and it is evident that Mr. Dibdin had not seen it, or it would have been enumerated in his List of Early Reviews. John Dunton, in his book entitled "The Life and Errors of John Dunton, late Citizen of London," gives the following account of the work.—"A third project of mine," says he, "for the promotion of learning, was a monthly journal of books printed in London, and beyond sea, called the 'Complete Library.' This design was carried on about ten months, when Monsieur La Crose interfered with me in a monthly journal, entitled, 'The works of the Learned,' upon which I dropped my own design, and joined with La Crose's bookseller in publishing his work."

It would swell this paper too much to give a list of the several books which are reviewed in this monthly publication; but it may be said that each number contains the account of one or more valuable works, which it is not likely will be found to be reviewed in any other journal. It also contains some biographical notices, the first of which is an account of the life of the Reverend Thomas Brande, by Dr. Samuel Annesley; and each number closes with a chapter entitled "News of Learning," containing literary intelligence from various parts of Europe.

Each number has a neat title-page, with a curious wood cut upon it, representing a full-length portrait of the editor (a London divine) in a garden, with a bee-hive, and the bees regaling themselves with the flowers. On the top of the print is the motto "*Sic nos non nobis mellificamus apes,*" and beneath it this couplet,

> All plants yield honey, as you see,
> To the industrious, chymic bee.

In a corner of the same print is seen a student writing in a library, with this motto over his desk, "*Omnia in libris.*"

In the third volume this print is omitted, in consequence of the London divine having given up the editorship, and a print of a large Raven, copied from John Dunton's sign in the Poultry, is substituted.

IX. 1692. "THE GENTLEMAN'S JOURNAL; or, the Monthly Miscellany, consisting of (literary) news, history, philosophy, poetry, music, translations, *&c.*, to be continued monthly." London, printed, and are to be sold by Richard Baldwin, near the Oxford Arms in Warwick-lane. This work is in small quarto, and was regularly printed monthly, until three volumes were completed, one volume in each year. The first number bears the date of January 169½. The editor was *Peter Motteaux.* I conceive this book must now be very rare, for it is evident that Chalmers did not know it, or he would have mentioned it in his "Life of Motteaux." The title-pages of the latter numbers have a neat wood-cut of a bunch of flowers, with

this motto, " *E pluribus unum.*" This work has more the character of a magazine than a literary journal, but as it contains a monthly account of "news of learning," which includes a list of new books, and of works then in the press, it seemed to deserve a place in this collection *.

X. 1693. "MEMOIRS FOR THE INGENIOUS, containing several curious observations in philosophy, mathematics, physick, history, philology, and other arts and sciences. In miscellaneous Letters, by J. DE LA CROSE, Eccl. Angl. Presb. To be continued monthly." This work is in small quarto, and the first number was published in January, 1693. London, printed for H. Rhodes, at the Star in Fleet-street, and for J. Harris, at the Harrow in the Poultry, 1693.

The volume consists of letters to eminent men on literary subjects, with opinions of books; but the observations appear to have been chiefly confined to the sciences referred to in the title-page, as copied above. The sixth number closes with the following advertisement. " At last I have received something. A very curious paper that was left for me at the *Latin Coffee-house.* It is a prophetical brass cut of the New Jerusalem; and not knowing what to do with it, nor how to shew my gratitude towards the presenter, I have clapt it on the door of my closet, an honour which no almanac ever had. I could wish the ingenious would present the world, by my means, with something more suitable to the design of these *Memoirs;* and I am informed they would, were I in a fit state to reward them; I am sorry I am not in such a condition, for their sake and for mine. Those who shall be so generous as to send me any paper, are desired to direct them to my lodgings in Playhouse-yard, Blackfriars. And I promise them to let 'em appear

* There was a work entitled " THE GENTLEMEN'S JOURNAL," published in the following year, by Randal Taylor, the first number of which came out on the 23d July, 1693; but this must not be included in our list, as it was not a literary journal, but a work of a very different character; it being merely a periodical descriptive account of all the fortified cities and towns in Europe.

abroad, in the best form that I possibly can; which is all that I can do at present."

XI. 1694. "MEMOIRS FOR THE INGENIOUS; or, the UNIVERSAL MERCURY*. By several hands." This work commenced in January 1694, and was published monthly by Randal Taylor, near Stationers'-hall. In the preface the editors say, "We have put the word Universal in our title, because we have chosen the universe for our subject, with all that it contains. We don't intend to trouble the world with our own particular notions, but fairly to exhibit the sentiments of the learned, upon whatever is curious in all sorts of learning, if it be not contrary to religion, good manners, or the government. For arts and sciences, we desire to inquire into their rise and progress, decay or improvement, and the history of their inventors. To conclude, the reader may have in this monthly essay a compendious view of universal learning, and at a very small expense either of money, time, or pains, reap the fruits of the studies of the most learned in all faculties; so that he may be able to discourse rationally upon any subject, by reading a little once a month, in a book so small that it will be no burthen, and so very cheap, that the expense is next to nothing."

XII. 1694. "MISCELLANEOUS LETTERS, giving an account of the Works of the Learned both at home and abroad." To be published weekly, price 3*d.* each, quarto, for William Lindsey, at the Angel, in Chancery-lane. The first number of this work appeared on the 17th October, 1694. When the editors had printed ten weekly numbers, which

* To prevent mistake, it may be proper to remark, that a work, with a title somewhat similar, appeared in 1708, viz., "The British Apollo; or, Curious Amusements for the Ingenious. By a Society of Gentlemen." This book was published, two leaves at a time, every Friday. The first number appeared on the 13th February, 1708, and the last, March 26th, 1712. The whole makes three volumes in folio. It was not a review of books, but was designed for answering curious questions in science and literature.

finished the year 1694, the work was continued in monthly numbers. The first volume consists of 578 pages, besides a copious index. The three first numbers of Vol. II for the year 1696, occupy 96 pages, but as I have no more of the work, I cannot tell how much farther it extended. The title seems to have been chosen merely to distinguish it from other journals, for no part of it is in the form of letters, but the whole has the appearance of a modern review, except that they are usually in octavo, and this is printed in small quarto. The design of the publication is thus given in the Preface: "We don't intend a translation or collection of foreign journals, but to peruse both them and the authors themselves, that we may be able to justify what we advance concerning them; though at the same time, we would not be understood as if we had a design to play the Critick. Our method shall be to keep a medium betwixt a meer catalogue and a real abridgment; abridgments having a natural tendency to destroy good books; so that the reader is to expect an idea of all valuable books, printed either here, or beyond sea, by which he may understand the designs of the authors, the reasons of their undertakings, and the manner of their performance, with an account of their principal subjects."

XIII. 1694. "THE HISTORY OF LEARNING, giving a succinct account and narrative of the choicest New Books, with a translation of what is most curious and remarkable in foreign journals. Designed to be published frequently." This work was printed in small quarto, and the first number appeared in May, 1694. The editors of this work profess in their Preface, to give "A Translation of the Naratives of the Choicest New Books in Europe, as they are epitomized in the Foreign Journals; and thus to afford a general and succinct view of the Learning and Studies of the most refined writers." Having only one number of this work, which commences with an Account of the Life of Descartes, I cannot tell to what length it was extended.

XIV. 1699. "The History of the Works of the Learned, or an impartial account of Books, lately printed in all parts of Europe, with a particular relation of the state of Learning in each country. Done by several hands." London, printed for H. Rhodes, at the Star, near Fleet Bridge, &c., 1699—1710. This work is usually seen in 8 thick quarto volumes, but I have a copy in 12 volumes, containing 760 closely printed pages in each volume, and I understand that the copy in the British Museum consists of 12 volumes.

The first volume is introduced by a sensible preface, giving an ample account of the plan on which the work would be conducted, and some of the volumes have wood-cuts of apparatus, &c., on the letter-press.

Among the editors of this very valuable work may be mentioned Mr. Ridpath, of whom John Dunton says, "he is a considerable scholar, and was first designed for the ministry, but by some unfortunate accident or other, the fate of an author came upon him. He was very fortunate in engaging in the 'History of the Works of the Learned,' which was originally my own thought, and I published the first book of the kind under the title of the *Athenian Supplement*, and the next under that of the *Complete Library*. It was this ingenious gentleman (Mr. Ridpath), that invented the *Polygraphy* or writing engine, by which one may with great facility, write two, four, six, or more copies of any one thing upon so many different sheets of paper, *at once*. This engine being moved by the foot, while the hand guides the pens, it keeps the whole body in warmth and exercise, which prevents many of the usual inconveniences of a sedentary life, besides the time which the engine saves in dispatch*."

Since the above was written, a friend of mine has accidentally met with a thirteenth volume of the "History of the Works of the Learned" for the year 1711, in quarto, printed like the foregoing, and published by the same bookseller. From an inspection however, of this volume, it appears that the publication of the

* Dunton's Life, page 239.

work was suspended at the end of the 12th volume, as this contains only eight parts instead of twelve as heretofore; the first six months of the year being comprised in two numbers instead of six as formerly; and, on the back of the general title page of the volume we read an ADVERTISEMENT, of which the following is a copy:

"The Booksellers who are the Proprietors of this Work thought fit for some considerations, to suspend the publication of it for the last six months, but have now agreed to resume it; and that there may be no chasm in this HISTORY OF THE WORKS OF THE LEARNED, all material books that were published at home or abroad, during *January*, *February*, and *March*, 1711, will be accounted for in one 12*d*. book; and those for *April*, *May*, and *June*, in another; after which, the Work is to be continued monthly, as before."

I have ascertained that one other number, said to be for January, February, and March, 1712, and purporting to be the commencement of a 14th volume, was published, but whether it was further continued or not, I have no means of knowing.

XV. 1703. "BIBLIOTHEQUE CHOISE'E." In this year Daniel Le Clerc began to publish this work, as a supplement to the "Universal Bibliotheque," which he had formerly edited in conjunction with Mr. De La Crose. This work was continued by Le Clerc to the year 1714, and consists of 28 volumes 12mo. It was immediately followed by his *Bibliotheque Ancienne et Moderne*, which extended to the end of the year 1728, in 29 volumes. "These little volumes," says Mr. Chalmers, "contain a great mass of valuable materials, of critical disquisitions and bibliographical notices and memoirs, and well deserve a place in the library of every literary man." The public are indebted to them for the documents from which Dr. Jortin principally composed his Life of Erasmus."

It may here be proper to state, that in the year 1704, John Dunton commenced a periodical work entitled the "Post Angel," which he says he carried on for 18 months, but whether this was strictly a literary journal or not, I cannot tell, as I

have not been able to meet with it. Nor have I been able to see another work of this kind, entitled *Bibliotheca Curiosa*, which was published in 1709.

XVI. 1707. "MEMOIRS FOR THE CURIOUS." This work was published monthly in sixpenny quarto numbers throughout the years 1707 and 1708, and when these two years were completed, a general title was given, of which the following is an exact copy:

"A compleat volume of the Memoirs for the Curious, from January 1707 to December 1708, containing an abstract of the most valuable things that have been published both at home and abroad, in relation to Arts and Sciences. With the lives of the most eminent men deceased within that time, Prince Lewis of Baden, Duke of Devonshire, Lord Cutts, Lord Belhaven, Dr. Sherlock, Dr. Drake, Dr. Brown, Mr. White, *&c.*; Monsieur Bayle, Monsieur Du Hamel, secretary to the French Academy of Sciences. The whole interspersed with several discourses on trade, botany, mathematicks, *&c.* By several hands. London, sold by J. Morphew, near Stationers'-Hall, 1710."

The first volume which contains 404 pages, closes with a general index, and the following advertisement:

"Though we have made several promises to amend the errata that have been committed in this first volume, yet we have not been able, by want of time in the person appointed to correct the press. Once more promising, we hope to convince the world at last that they shall not find any such errors for the time to come; and, in consideration of which, (it being the only atonement the proprietors can make for past mistakes), any gentleman may have this first volume, sold for 6*d.* each, being twelve in number, for 5*s.* stitched, or bound at 6*s.* 6*d.*, which would otherwise have been 7*s.* 6*d.* In answer to our not punctually keeping time, we reply; that there being so many gentlemen voluntarily concerned, out of a generous communicative faculty, and not for lucre or livelihood, we were obliged to stay their time; but as they intend, so we promise on their words, to amend that too."

The second volume which extends only to 382 pages, closes with the number for December, 1708.

This work was either edited by James Petiver, the eminent writer on botany and other subjects of natural history beforementioned, or he was a great contributor thereto. It is now, I believe, scarce; I know of no copy except the one from which I have transcribed the above, and that is in the library of the late Sir Joseph Banks. A third volume was advertised to be published, but I cannot tell whether it was ever printed or not.

It would be culpable if I were not to say something of the extraordinary editor of this work. The following, however, must suffice.

James Petiver who was originally an obscure apothecary in Aldersgate-street, became so eminent by his industry in botanical pursuits, as to be the early and chosen friend of Sir Hans Sloane, who was one of the pall-bearers at his funeral. A newly discovered plant was also named after him, and I find, on consulting Dr. Thomson's history of the Royal Society, that he was elected a fellow thereof, under the designation of apothecary to the Charter-House, on the 27th November, 1695. This great naturalist was in the habit of engaging captains and surgeons of ships to bring home specimens for him, and he directed their choice and enabled them to judge in some measure of proper objects, by distributing *printed* directions among them, with folio books of waste paper, to contain the specimens they might collect for him. Mr. Millan the bookseller, who about the year 1760 came into possession of the remaining copies of his works in folio, used to bind up one of these original instructions, printed upon coarse paper, with each set. It runs thus: "JAMES PETIVER HIS BOOK, for a collection of whatever *trees*, *shrubs*, *herbs*, *grasses*, *rushes*, *ferns*, *mosses*, *sea or river-weeds*, &c., you shall find." Then follow "Directions for the gathering of Plants," which are very circumstantial and curious, and each is signed with his name. By these means he collected so valuable a museum that Sir Hans Sloane a little time before his death offered him four thousand pounds for it,

and afterwards purchased it; and the collection now makes a part of the British Museum. The plant which was named after our author was the *Petiveria*, of the class and order of *Heptandria Monogynia*. It being then a newly-discovered plant, the genus was dedicated, by Plumier, with many compliments, to Mr. Petiver. See *Pultney's Historical Sketches of the Progress of Botany*, Vol. II, 31-43; Sir James Edward Smith's *Correspondence of Linnæus*, &c. Vol. II, 161, *&c.*

XVII. 1709. "CENSURA TEMPORUM." The Good or Ill Tendencies of Books, Sermons, Pamphlets, *&c.*, impartially considered, in a Dialogue between Eubulus and Sophronius." Printed for H. Clements, at the Half Moon, in St. Paul's Church-Yard, Vol. I, for the year 1708. "This work was published monthly in small quarto, 32 pages in each number, and the first volume containing 384 pages concludes with December, 1708. A preface to the first volume was given with the last number, which concludes thus: "If any of my readers shall at any time have committed to paper any notes or remarks upon new books, or old ones reprinted, which they are willing the publick should have the benefit of, they would be pleased to transmit them to the bookseller for the author of these papers, who will take care to have them printed, and annext as an appendix to the dialogues, with or without mention of their names as they shall direct, and be ready to return his thanks."

In the last page of the volume is an intimation that the work would be resumed with the new year, but how long the publication was continued I have not been able to learn.

About this time, *viz.* in 1707 and 1708, a book containing much interesting matter was published, entitled the "PHŒNIX, or a revival of Scarce and valuable Pieces, no where to be found but in the Closets of the Curious." In two volumes octavo; but, as this work was not printed periodically, it does not properly belong to our class.

XVIII. 1710. "MEMOIRS OF LITERATURE, containing a large Account of many valuable Books, Letters, and Disserta-

tions upon several subjects, Miscellaneous Observations, *&c.*" In eight volumes. The second edition, revised and corrected. Octavo, London, 1722. In March 1710, Michael De la Roche began a work entitled "MEMOIRS OF LITERATURE," and he continued it in four volumes, to September 1714. The first of these was in *folio*, the three others in *quarto*. Neither of these are now often seen; but the work was reprinted in the year 1722, in 8 vols. octavo, and this is the book which is generally found in the libraries of the curious. In the preface to this impression, the editor writes thus:

"The first edition of these papers being very scarce, I have reprinted it with several improvements. This new edition consists only of three hundred and fifty copies; so that this work will always be uncommon." A copious table of the contents is prefixed to each volume, and a large index to the whole closes the eighth volume. This work is so full of entertainment and instruction, that I have always considered it to be one of the most valuable of our literary journals. The variety of interesting matter is so great, that it would be very difficult to satisfy oneself in making extracts. I am, however, desirous of observing, that these volumes contain a more full and circumstantial account of the character and death of Nicholas Anthoine, who was strangled and burnt in 1632, for having left the Christian religion and embraced Judaism, than can be found elsewhere. This work also contains a more particular account than I have ever seen, of the writings of the learned Michael Servetus, who discovered the circulation of the blood *, and who was burnt alive at Geneva, in the year 1553, by the intrigues of Calvin, for the opposition which he had given to the prevailing religious doctrines of the times. As the works of this great man are now extremely scarce, in consequence of the copies having been collected and burnt by authority at Vienna and Frankfort, the facts which Michael

*Dr. Wotton tells us that Servetus announced this discovery in a work entitled *Christianismi Restitutio*, which was first printed in 1553, the very year in which the author was burnt. See Wotton's Reflections on Ancient and Modern Learning, 1705, page 215.

De La Roche has preserved, render these volumes highly valuable.

XIX. 1717. "Bibliotheque Angloise, ou Histoire Literaire de la Grande Bretagne, par M. D. L. R.—Amsterdam, 1717—1719," in small 24mo., (M. D. L. R. meaning Michael De La Roche.) This work was printed in Holland, and is complete in fifteen volumes. The editor printed two half-volumes a-year; the first volume was published at Amsterdam in 1717, in the French language, and consists of 544 pages, besides two Tables of Contents. The fifth and last volume by La Roche, was printed in 1719. At this time his Dutch publisher abandoned him, and chose a new editor of the name of Armand de la Chapelle, who edited ten volumes more, and of whom nothing appears to be known. In consequence of this, Michael de la Roche was under the necessity of publishing his labours under a new title. From this time, therefore, he gave his papers to the world under the title of "Memoires Literaires de la Grand Bretagne." The new bookseller whom La Chapelle employed, afterwards commenced another similar work which he published under the title of "Bibliotheque Britannique," in 25 vols. 12mo. from the year 1733—1747.

XX. 1720. "Memoires Literaires de la Grande Bretagne, par Michael de la Roche, Auteur des cinque premiers tomes de la Bibliotheque Angloise." 24mo.

Mons. de la Roche continued to publish this small work under the above title for five years, making sixteen parts in the whole, which he calls volumes. These are, however, usually bound two volumes together, like the Bibliotheque Angloise; two of these volumes, or four parts, making a year together. The whole is neatly and uniformly printed, and for the convenience of their being bound two volumes in one, the two volumes are paged, one after the other, in one regular series of progressive numbers. That this work is complete in 16 volumes is evident from the last volume being marked, "*Fin du XVI., et dernier Volume.*"

I have understood that the *Bibliotheque Angloise*, (No. XIX.) was published by Mons. De la Roche as a continuation of the Memoirs of Literature which had been published by him some years before, and that this, together with the *Mémoires Literaires de la Grande Bretagne*, supply the deficiency which there would otherwise have been between the "Old and New Memoirs," the latter of which (*see* N°. XXII.), were begun on the author's return to this country.

[To be continued.]

Art. IV. *On the Economy of Fuel, as connected with the Improved Methods of heating Steam-boilers, and burning Smoke, invented by* Messrs. Parkes, *of Warwick.*

The experiments on the evaporation of water, given in the annexed table, were performed at three distinct establishments to determine, to the satisfaction of the proprietors, the value of the method of firing steam-boilers and consuming smoke, introduced by John Parkes and Sons, of Warwick.

To satisfy the man of science as well as the practical engineer, that their method possesses real advantages over that in common use, it was necessary that a trial should be resorted to, capable of demonstrating the facts with the fewest possible chances of error. This proof has accordingly been obtained with several kinds of fuel, and at boilers of various dimensions, the same test being employed in each instance, *viz.*, the comparison of the fuel burnt, with the quantity of water converted into vapour. Such a mode of decision was clearly preferable to that of merely comparing the quantity of coal employed to work a steam-engine, when fired on the old and new plans; since the variation of the weather, with other changes incidental to the engine itself, and to its load, might defeat all attempts at accuracy.

The experiments exhibited in the Table, besides establishing the economy of the system, point out several facts worthy of attention.

The boiler employed at Messrs. Horrocks's and Co. is very nearly of the same content, and has nearly the same surface of water and flue, as the one used at Messrs. Thomson and Co's., though of different dimensions as to length and width. It will, however, be perceived that the number of cubic feet of water, evaporated in a given time from the former, exceeded the number raised from the latter in the proportion nearly of two to one. This excess in the power of producing steam, was derived from two causes; the superiority of the draught of the chimney, and the greater width of the boiler, which permitted a much larger quantity of fuel to be brought into action, and presented a corresponding increase of surface to receive the radiant heat of the fire. It will be likewise seen by comparing experiments, Numbers 9 and 10, that the greatest performance in point of time occurred, without any material reduction in the effect derived from each pound of coal.

To printing, dyeing, and bleaching works, which require irregular and sudden supplies of steam, a chimney so lofty and capacious as to make the boilers answer the demand upon them with certainty and ease, should be an object of the first importance, since a deficiency of steam is a source on some occasions, not only of delay, but of real injury to the operations there carried on. The utility and economy of such a structure are forcibly illustrated by these experiments; and they also shew that the proportions of the boiler are not unworthy of attention.

The experiment, N°. 6, at Primrose, was made to determine on what circumstances the saving produced by John Parkes and Sons' method depends. By comparing experiment N°. 6 with N°. 7, it will appear that the economy of the plan arises, chiefly, from the perfect combustion of those portions of carbonaceous and gaseous matter, which commonly escape in an unconsumed and unprofitable state. It is to be observed, that the results of these experiments do not exhibit the relative strength of the different kinds of coal, but simply the comparative effect of the old and of the new methods of using the same kind of coal under the same boilers.

The experiments are stated in two modes, so far as respects the temperature of the water with which the boilers were supplied; the first expresses the real temperature at the time of making the experiment; the second exhibits the proportion of coal required to evaporate the water, supposing the latter to have entered the boiler at 212°. These latter columns are given, in order that the results of the several experiments might appear upon equal terms, which would not have been the case, unless the water which entered the boiler in the different experiments, was reduced to an uniform temperature. This temperature I have fixed at 212°, because it is at that degree of heat that water begins to be converted into elastic steam equal to the pressure of the atmosphere. In order to separate the quantity of coal burnt in heating the water to 212° from that actually spent in evaporating it. I have assumed Mr. Watt's figures for the latent heat of steam.

In the eight first columns of the Table, the facts and inferences from each experiment are stated. In the three last the results are given as deduced from the theory, that a just comparison may be made between the several experiments. This comparison appears in the last column. An example (*a*) is also given of the operation by which I obtain the figures in the three last columns, which are therefore subject to verification, should the method employed be found objectionable.

Manchester, *Feb.* 26, 1822. JOSIAH PARKES.

TABLE.

	No. of Experiment.	Plan.	Weight of Coals burnt.	Weight of Water evaporated.	Time.	lbs. of Water evaporated by 1 lb. of Coal.	Cub.Ft. of Water evap. by 112 lbs. of Coal.	Temp. of Water on entering the Boiler.	Wt. of Coals burnt to raise the Water to 212°.	Wt. of Coals burnt in evap. from 212°.	Cub. Ft. of Wat. evap. by 112 lbs. of Coal from 212°.	
At Messrs. Thomson, Chippendall and Co., at Primrose.	No. 1	Old	lbs. 2576	'lbs. 12956	Ho. Ms 11 30	lbs. 5.02	Cub. ft. 9	° 42	lbs. 391	lbs. 2185	Cub.Ft. 10.62	
	2	New	2576	14356	11 25	5.57	10.	42	391	2185	11.77	
	3	ditto	1568	9318	9 0	5.94	10.65	42	238	1330	12.55	
	4	Old	1568	11468	8 0	7.31	13.1	44	235	1333	15.4	Boiler 20 feet by 5 feet 6 inches, Grate 4 feet 6 inches, by 4 feet 6 inches, flue through the Boiler.
	5	ditto	1568	11512	8 46	7.34	13.1	44	235	1333	15.47	
	* 6	......	1568	11668	10 0	7.44	13.45	44	235	1333	15.68	
	† 7	New	2016	16775	12 25	8.32	14 92	44	303	1713	17.54	
At Mess. Horrock's & Co., of Preston.	8	Old	2576	19312	9 19	7.5	13.4	76	322	2254	15.35	Boiler 13 feet 6 inches by 8 feet, Grate 4 feet 6 inches by 6 feet 2 inches, flue thro' the Boiler.
	9	New	2576	21875	10 0	8.48	15.2	80	314	2262	17.33	
	‡10	ditto	2688	22125	8 0	8.23	14.75	74	341	2347	16.93	
At the New River Head Water-Works, Islington.	11	Old	3200	22500	10 59	7.03	12.63	102	332	2868	14.06	Two Boilers 15 feet by 5 feet, Grates, old plan, 3 feet 10 inches by 4 feet, new do., 4 feet 6 inches by 4 feet 6 inches, no flue through the Boilers.
	12	ditto	2312	16125	8 0	6.97	12.5	101	241	2071	13.95	
	13	New	1917	15375	7 18	8.02	14.37	96	210	1707	16.14	
	14	ditto	1837	14764	6 54	8.03	14.4	97	198	1639	16.13	
	15	ditto	2199	18112	8 10	8.23	14.75	97	237	1962	16.51	

* In this experiment the grate was charged, but the smoke allowed to pass off unconsumed

† In this experiment the smoke was burnt.

‡ In this experiment the full force of the draught of a very powerful chimney was given to the fire first seven hours.

(*a*) Example, see experiment first. Latent heat of steam, 950° Watt. 212—42°=170°—170°×950°=1120°.=sum of sensible and latent heat: then as 1120° : 2576 lbs. : : 170° : 391 lbs. and 950° : 2185 lbs.

Art. V. *Observations and Experiments upon the Chemical Composition of the Seeds of the Croton Tiglium, and the Oil which is procured from them. By* John Nimmo, M.D.

[Communicated by the Author.]

On taking a review of the various substances which have been employed, at different periods of the history of medicine, for the cure of diseases, the frequency with which the opinions of medical men have changed in regard to their presumed efficacy, cannot fail to arrest the attention of the most superficial observer. In regard to many substances which have been employed during an early period of the science of medicine, the discredit into which they have fallen subsequently may be traced to the discovery of other medicinal substances, the greater efficacy of which, or convenience in administering them, has rendered the employment of preceding remedies less frequent, and at length succeeded in degrading substances which were resorted to with confidence, to that of being considered of no value whatever. Thus has hellebore fallen so low in the estimation of the medical profession, from a pre-eminent station which it once held in the cure of some of the most important diseases; most of the substances employed to produce vesication have given way, soon after cantharides was introduced into general use; thus have ipecacuanha and tartarized antimony become substitutes for other remedies; and many other instances might be adduced, from which it would appear evident, that to other principles than caprice, alteration in the nature of diseases, and changes in the constitution of mankind arising from difference of climate or habits, must the change in opinion relative to the variable efficacy of different medicinal substances be attributed.

The seeds of the *Croton Tiglium* have been known to be possessed of medicinal efficacy as a purgative, nearly nine centuries. At no period, however, do they appear to have been

employed generally in the practice of medicine in Europe. The disagreeable taste, excessive acrimony, and the violence of their operation, presented objections to their employment in medicine; and the danger which at times occurred in using them, produced an impression so unfavourable, that they came to be seldom used, and very soon, as if by common consent, they were banished from medical practice. Such was the state of medical knowledge relative to them, that few medical men had any practical acquaintance with them, and such as had read about them were led, by the statements which they perused, to view them as a remedy possessed of the highest degree of virulence, such, indeed, as no men could be justified in resorting to in practice without danger to their patients, or incurring the risk of injury to their professional character, by employing a remedy of so doubtful a nature. The commendations which of late have been bestowed upon the croton oil, and a sufficient quantity having been imported into this country for making trial of its efficacy, have had the effect of reviving its use in medicine; and already have many valuable communications appeared in various periodical journals, by means of which the general sense of the medical profession will soon be collected. Either it will be received into modern practice, as a powerful remedy for the cure of various diseases, or the opinions which contributed to banish it out of the field of medicine will be confirmed, and again subject it to the same general proscription which formerly it seemed so much to deserve.

A considerable time ago I procured twelve drops of this oil, for making a few trials of its efficacy, and would probably have used it in the manner recommended upon the label, but for an economical reason, namely, to obviate the loss which would be sustained by dropping, out of a large phial, the few drops I had obtained. Recollecting the solubility of some of the fixed oils, particularly castor oil, in alcohol, I poured upon the croton oil two drams of alcohol sp. gr. .825: by agitation I perceived that a partial solution took place; pouring this off, two drams more of alcohol were added, by which an additional

portion was dissolved. A third quantity seemed to have little effect; and even when it was aided by heat, though a slight appearance of solution took place, the whole was precipitated when it became cold; and there was left an oily-looking substance, which appeared to be equal to somewhat more than a third part of the original quantity of oil operated upon. On tasting the alcoholic solution, I found it possessed of the characteristic acrimony of the oil: the undissolved portion had none whatever. This solution of the active ingredient of the croton oil I considered preferable, as a medicine, to the entire oil, as obviating the following objections; namely, the difference of dose in consequence of the inequality in the thickness of the lip of phials, the greater or less degree of viscidity in the oil at different temperatures, and the difficulty of apportioning the dose to difference of age or constitutional susceptibility to the action of the ordinary purgatives.

The rude analysis, practised above, shewed that the croton oil was not a simple substance, but a binary compound, at least; one part consisting of the purgative principle, for in administering the alcoholic solution in doses relative to the number of drops decomposed, the same effects were produced as have been attributed to the entire oil; the other part inert, for having first tasted, and then taken, the whole of the residuary oily substance, I found it entirely devoid of acrimony, slightly rancid, and possessed of no power of moving the bowels.

The supply of croton oil from London having been quickly exhausted, and wishing to prosecute the investigation of its properties by an extended trial, I was informed that a gentleman in town was possessed of a small quantity of the seeds, with which he was so kind as to supply me; and with the view partly of obtaining a chemical analysis, but principally to procure some of the oil, the quantity of which in the market was reported to be nearly exhausted, the following series of experiments upon them was instituted, which perhaps will not be considered devoid of interest in a chemical point of view, and of utility in the medical administration of this powerful remedy.

Having weighed a quantity of the seeds, and separated the kernel from the shells, I found 100 parts contained 64 of the former and 36 of the latter. Digesting a quantity of the latter with alcohol a sufficient length of time, though a dark-coloured tincture was obtained, on tasting it, it was possessed of no acrimony whatever. It is stated, indeed, that they possess in a considerable degree the peculiar property of the seeds, but in the present case no such acrimony existed. The attention was consequently directed solely to the kernel of the seeds.

Taking 40 grains of the kernel, they were first reduced to a paste by bruising them in a mortar, upon which alcohol being poured, they were digested with a moderate heat several days. The whole being poured upon a filter, the fluid parts passed through, and upon the insoluble part more alcohol was poured, until every thing which was soluble in that fluid was washed away. The residuum was dried, and weighed 29 parts, 11 had been dissolved; the former tasteless, the latter resembling the alcoholic solution obtained from the entire oil. The undissolved residuary matter showed evidently that it contained a fixed oil, which tinged the paper in which it was dried by heat. To discover the proportion in which this oil existed in the kernels not soluble in alcohol, it was treated with purified oil of turpentine, a substance which has not been employed in chemical analysis of vegetable substances, but which in some instances may be found advantageous.

Before, however, going on to detail the action of the purified oil of turpentine, it is necessary to remark that, in its ordinary state, it is not sufficiently pure to be employed for this or similar purposes; for even when recently distilled, it contains a substance which, when the oil is evaporated, remains behind, and would in the present instance have led to a false estimate by the addition of weight to the residuary undissolved substance. To remove the whole of this, I have employed alcohol to purify the oil of turpentine for medicinal use, without diminishing its efficacy, but greatly lessening its disagreeable taste, and its injurious action upon the kidneys, as well as for chemical purposes. To eight parts of the oil add one part of the

strongest alcohol, and let them be well agitated; in a few minutes a separation takes place; the oil, unless very impure, falls to the bottom, and the alcohol, having dissolved the impurities, floats at the top. Pour off the alcoholic portion, add an equal quantity of alcohol as before, agitate and separate the liquids. If this be repeated three or four times, the oil becomes nearly tasteless, almost without smell, and when a portion of it is evaporated, it leaves no residue. It is necessary to remark, that, pure as the oil may be rendered, it speedily undergoes alteration, and returns to its original state of greater or less impurity.

Upon the 29 parts of matter which remained after the action of alcohol had been exhausted, the purified oil of turpentine was poured; and after digestion a suitable length of time, the whole was thrown upon a filter, washed with more of the oil, and the residuum thoroughly dried. Of 29 parts there remained 16 parts undissolved, 13 parts were dissolved. These proportions being brought to 100 parts, the following is the estimate:

Croton seeds 100 =	27.5	acrid matter soluble in alcohol,
	32.5	fixed oil soluble in oil of turpentine,
	40.	farinaceous matter insoluble by both.
	100	

In another experiment, the alcoholic solution, being filtered hot, carried off 34 parts, leaving 26 for the action of the oil of turpentine, and 40 parts, just as before, undissolved. In other experiments the results were nearly in the proportions above stated.

Upon 100 parts of the bruised kernels, oil of turpentine was poured, and treated as before, yielded—

60	parts of the acrid matter and oil soluble in it,
40	parts of insoluble farinaceous substance.
100	

The action of sulphuric ether was next tried, and proved to be an equally good solvent of these peculiar principles with the oil of turpentine; over which, indeed, it possesses some advantages, in the view, particularly, should it ever be found necessary, of procuring the substance in the utmost degree of purity, and at no great price.

Digesting sulphuric ether upon 100 parts of the bruised seeds, throwing the whole upon a filter, covering it closely during the process of filtration, and washing the residuum with a sufficient quantity of ether, it was found to weigh 40 parts, 60 having been dissolved. By this process from 300 grains of the seeds, from which if 102 grains are deducted for the shells, there are left 198 grains of the kernels, I obtained upwards of two drams by measure of an oil which possessed all the qualities, as to taste and medicinal efficacy, which the purchased specimen contained.

It cannot be doubted that the oil thus prepared must be as genuine as that procured by torrefaction and expression of the seeds, as practised in India; and in some respects, perhaps, it may be preferable, as tending less to occasion a decomposition of the active ingredients of the seeds, by the heat applied to effect the separation of the oily portion from the farinaceous part. A circumstance of this kind may be the cause of a considerable difference in the quality of two specimens of the oil which I have seen, in which the colour and fluidity are not the same, besides a difference in their relative strength as purgatives, without supposing the existence of any fraud on the part of those persons who have it for sale.

To determine more accurately than by any of the former experiments, the relative proportion of the acrid purgative principle to the fixed oil, it seemed necessary to adopt the following plan. From the first, compared with the second experiment with alcohol, it was remarked that heat rendered more of the oil soluble than when the fluid was cold; and it is obvious that the apparent quantity of oil must have been diminished by that portion which the alcohol was able to dissolve at the ordinary temperature of the atmosphere. Using a weaker alcohol, of sp. gr. .834, into which there was poured a quantity of olive oil, to which the insoluble part of the croton oil bears the strongest resemblance, agitating, besides heating them, together, that the alcohol might become saturated with the oil; upon 40 grains, (or 84 drops) of the oil, which had been extracted from the seeds by ether, this alcoholic solution of oil was poured in successive quantities. A bright yellow-coloured

solution of the acrid principle was thus obtained :—the insoluble portion weighed 22 grains, the dissolved part was, of course, 18 grains.

From the preceding data, the composition of the kernels of the croton is the following :

27	of acrid purgative principle
33	fixed oil
40	farinaceous matter
100	

The oil itself is composed of

45	acrid principle
55	fixed oil
100	

When the alcoholic solution was dropped into solution of litmus, it turned it red, shewing the presence of an acid; but when a solution of alkali of known strength was added, the quantity was found to be so extremely small, that its nature could not be ascertained without destroying more of the oil than was in my possession. When poured into water the mixture becomes nebulous; when passed through filtering paper, the precipitate is retained by it; the clear water which was procured from a proportion of the alcoholic solution, containing four doses of a sufficient strength to produce a purgative effect, being taken, had no effect in moving the bowels. The acrid substance appears, from this and other circumstances, to reside in a resinous principle, which is soluble in alcohol, sulphuric ether, volatile and fixed oils.

From the difference of effect which has been noticed by those who have examined the action of the croton oil, there is room for suspecting that, in many instances, additions have been made to the real oil, and to such practices it is manifest there is a strong temptation from the high price at which it is sold, and the facility with which adulteration can be practised without any apparent means of detection. The observations and experiments stated above, it is hoped, will be found to furnish the ready means of detection. Let a very light phial be counterpoised in an accurate balance, pour into it 50 grains or more of the croton oil, add alcohol which has been digested upon olive oil, of which it dissolves so little as not to injure,

in the smallest degree, the alcoholic solution for subsequent use, agitate well, pour off the solution, and add more alcohol in the same manner until the dissolved portion is diffused in such a proportion of alcohol that each half dram measure shall contain equal to one dose of the croton oil for an adult,—by placing the phial near a fire, to evaporate what remains of the alcohol in the bottle, if the remainder be to that which has been abstracted by the alcohol as 55 to 45, the oil is genuine,—if olive, or any other oil, little soluble in alcohol, has been added, the residuum will be in larger proportion. But if castor oil has been employed, the proportion of the residue will be smaller than in the genuine medicine.

The alcoholic solution appears to me the best vehicle for administering the active principle of the croton oil; and furnishes the means of readily proportioning the dose to the various circumstances of cases under treatment. The following is the formula which I have employed, and the directions for avoiding the uneasy feelings which are produced in the mouth and throat.

R. Alcohol. croton, ʒ ß.
Syrupi simplicis.
Mucilag. gum. Arab. $\overline{a\,a}$ ʒ ii.
Aq. distillat. ℥ ß. M.—ft. haustus.

After swallowing a little milk, take the draught very quickly, and wash it down with repeated quantities of the same diluent.

Having already administered this remedy in private practice, in considerably above an hundred doses, and to some patients many times, a copious list of cases might be furnished. At present it may be sufficient to state that in not more than three or four cases was vomiting produced, and that not in a violent degree;—in not many more was nausea felt;—in all the cases purging was induced in a space of time between half an hour and three hours after taking the medicine:—the purgative effects were generally moderate, accompanied with griping, rarely; and in proportion generally to the effect which was intended to be produced, circumstances which some of my medical friends, whom I furnished with the alcoholic solution prepared from the seeds, as well as myself, are inclined to attribute to the instantaneous and equable diffusion of the

active principles of the croton seeds over the inner coat of the stomach and abdominal viscera when the above formula was employed; while, on the contrary, it must unavoidably happen that the oil, taken merely mixed with any fluid, made up into the form of pill with any substance, or diffused with sugar or starch, may, at times, be applied, in a concentrated form, to a particular part of the stomach or intestines, and excite an action such as to occasion nausea and vomiting in the former case, and spasmodic action, with pain and hypercatharsis, in the latter. When the unfavourable circumstances last mentioned occur, there is no doubt much reason to fear that this purgative may excite inflammation or induce a state of greater or less debility. Among the cases I have had to treat was one of a lady, who, after using diuretic medicines of the most powerful kind, and undergone a course of mercurial inunction for the cure of abdominal dropsy, was rapidly sinking under an accumulation of the dropsical fluid, and nearly a total loss of appetite and strength; her state being almost hopeless, the mixture of alcoholic solution of the croton was administered at first with extreme caution, and afterwards, when it was found she could bear it, in augmented doses, so as to cause three or four evacuations of the bowels daily, with the most beneficial consequences; by augmenting the appetite and the strength, and by the discharge of watery stools, the speedy reduction of the size of the abdomen became apparent. After two weeks' use, the irritability of the stomach made it necessary to stop the use of it:—the complaint shewed strong symptoms of returning;—diuretics were then employed, but the effect which they had upon the appetite and in inducing debility, soon made it necessary to desist:—the croton mixture was again used, and by employing opiates and carminatives, she was enabled to continue its use until a cure was accomplished. In the cure of delirium tremens, I have found it a powerful auxiliary to opium; in one case of this disease, arising from excessive intemperance in the use of ardent spirits, in which the most distressing phantasms appeared, the wildest imaginations were produced, and sounds heard, the immediate effects

of a full dose of the croton solution were very apparent on one occasion, and equally so on a subsequent attack from the same cause. In another case, where similar symptoms existed, but not from the same cause, in a gentleman from the West Indies, and most probably the precursor of mania, it was equally beneficial. In this case, though a person of a very highly cultivated mind, there were present all the symptoms which have been described as arising from the belief and superstitious dread impressed on the minds of negroes in West India plantations from the spells and incantations of *Obi*, or African witchcraft. In all cases in which there is a superfluity of the secretion of bile regurgitating into the stomach, it is of service unless the medicine itself is rejected by vomiting; and in the removal of jaundice from obstruction or spasms in the bowels, (perhaps equally in those of the biliary passages,) it proved most beneficial and agreeable in its effects, as reported to me by the gentleman who furnished me with the seeds. In a case of a child, about two years of age, with almost total want of appetite, and labouring under what appeared to be an incurable state of tympanites intestinalis, it was prescribed in doses to cause two or three stools daily. In four days the child, belonging to poor parents, was brought back to me, with an appearance so much improved, in so short a period of time, as scarcely to be credible; the return of appetite, and a liveliness of countenance, and such a reduction in the tympanitic symptoms, that I formed the most sanguine hopes of ultimate recovery. Though the mother made many promises to return again, she neglected them, and though no determinate conclusion can be drawn from the early symptoms of amendment in this case, which was truly hopeless at first view, I am induced to think, that, in the cure of this most stubborn complaint, much benefit may be expected from the cautious, but steady, use of the croton oil. In a case of excessive corpulence, with the most alarming symptoms of determination to the head, amounting at times almost to apoplexy, a few doses of the solution produced the most signal benefit; and, without having had occasion to let blood, every symptom was removed which could have

been expected from venesection, and the patient being, forthwith, put upon a regulated system of diet, exercise, and the occasional use of the croton mixture, the size of the body is slowly, but progressively, diminishing; exercise can now be taken without hurrying the breathing, the mind has regained its accustomed activity, and if the plan be steadily persisted in, there is no reason to dread that even the natural tendency, in this case, of the constitution to produce too much blood, and the deposition of an excessive quantity of fat, at the early period of thirty-two or thirty-three years, may be kept at bay, and life preserved.

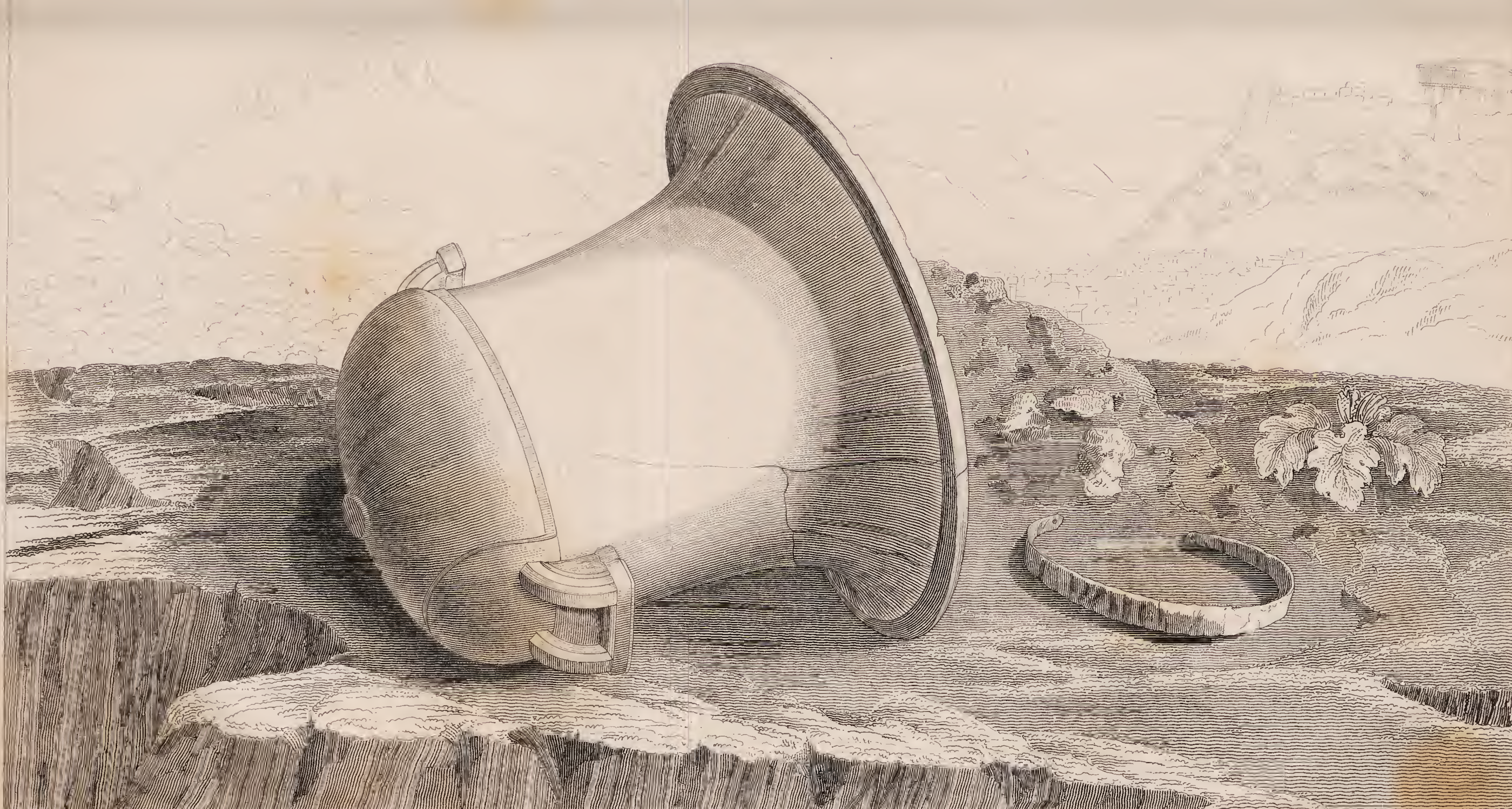
Glasgow, *4th March*, 1822.

Art. VI. *An Account of a Cinerary Vase, found at Athens. By* Lewis Vulliamy, Esq., *late Travelling Student of the Royal Academy.*

The vase, the subject of this paper, is a simple, but beautiful, example of the taste of the ancient Greeks. It contained, amongst the earth with which it was filled, pieces of burnt bones, a golden fillet, and some pieces of gold, which preserved no intelligible form. This vase is of fine Pentelic marble, and is wrought with the chisel in a masterly manner, but its execution is little advanced beyond the state called *abbozzato* by the Italians, and *boasted* by the English sculptors. The inside is worked in a similar manner. Its average thickness throughout is about half an inch.

Previously to describing the manner in which the vase was found, or the circumstances of its situation, it will be proper to observe, that, for some years before my visit to Athens, it had been greatly the practice with travellers, and also with some of the inhabitants, to dig for antiquities, and in the depositories of the dead they had the best chance of success.

The marks of their curiosity, avarice, or antiquarian zeal, presented themselves in all directions, but especially on the road to the Piræus, and that to Cape Sunium, in graves broken into, and after having been rifled of their contents, left with the fresh turned-up earth and fragments scattered round.



L. Vulliamy del.^t

J.^s Basire sc.

Cinerary Vase found in a Cemetery near Athens.

Published by John Murray, Albemarle Street, London 18[illegible]

This practice the Turks had lately forbidden, not out of regard to those whose remains were thus disturbed, but in order to prevent the quarrels which constantly arose amongst the Christians about the division of the treasures thus discovered.

The interference of the Turks would alone have proved a great obstacle to my exploring below the surface of the earth, but neither did the object of my journey admit of employing time in this way, at least not unless it had been to trace the plan, or discover some of the architectural details not apparent above ground, of a temple or some other edifice.

It was thus, in the search of objects connected with my professional pursuits, that one afternoon (Sunday, January 16,) I walked out with my companion, Captain Jones, to explore the remains of antiquity which still existed in the vicinity of Athens, our attention till then having been chiefly engrossed by the many objects of interest within the walls.

After having examined some masses of masonry, and remains of sepulchral monuments, and having found little to reward the search, except the architectural fragments which are so frequently found built in the walls of the churches and *metochi**, we came to a place of sepulture, the first on the right of the principal road from the city to the Piræus, which road issues from Athens at the north-western gate, and is soon joined by several branch roads from the other gates. These places of sepulture continually occur by the sides of the great roads, and are formed in situations where the rock rises generally from three to five feet above the surface of the ground. In this rock the graves are excavated, in the form of parallelograms, large enough to contain the body, generally with a groove or rebate round the top, to receive the massive stones that covered them.

While walking upon this cemetery, conversing with my companion upon the extraordinary people whose remains had once occupied them, and amusing ourselves with conjectures on the

* The metochi are frequent all over Greece; they consist of farm buildings, a church and residence for the priest or caloyer, and for the despota, or governor of the establishment under the convent, to which, with the surrounding land, it belongs. These and all other churches are erected on the sites of ancient temples, many of whose fragments are inserted in their walls.

former tenant of the one upon which I then stood, I raked with my stick amongst the mould which filled it, and felt the resisance of a hard substance. Though I little expected that a treasure so precious should thus present itself, unsought, to my notice, I nevertheless cleared away the surrounding impediments of earth and stones, and soon was rewarded for the search, by seeing gradually revealed a vase of classic form.

We were not a little pleased with the discovery, but were rather at a loss to devise means to secure our acquisition from the dangers of Greek treachery, and Turkish rapacity. We at length decided that our safest course was again to cover the vase with the earth, under which it had doubtlessly lain concealed for many centuries, to observe a prudent silence respecting the discovery, and to return the following evening almost at night-fall, with instruments for excavating under our cloaks, and after raising it from its bed, to convey it to a place of security, trusting to the cover of darkness for protection under our operations. Our task was not accomplished without considerable fatigue, owing to the weight of the vase, and the uneven, broken ground over which we had to pass to get to our dwelling, for we avoided the high road and frequented paths to escape observation. My friend Captain Jones kindly assisted me in supporting it suspended on a short pole, of which each end was borne by one of us. Safely lodged in our habitation, it was soon visited by all the travellers, and by the European consuls resident at Athens, some of whom are the principal antiquaries of the place.

These gentlemen said the form of the vase was similar to that of the cups of Bacchus, and that the golden fillet was such as was worn round the head by the Athenian females before marriage. They were much surprised and pleased at the discovery, but were rather jealous of its having been found with so little trouble. The celebrated M. Fauvel, consul for France, said, " *Monsieur, vous étes trop heureux.*" Another consul mentioned the time and money he had spent in excavating, without making any discovery of so much value.

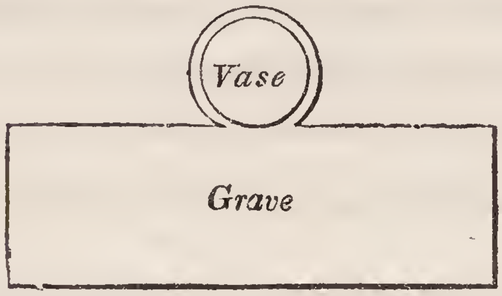
But the fact is no one had ever thought of seeking in the place

in question, for there it was evident that the graves had been *already* ransacked, and their contents removed; even the flat stones which had covered them had been taken away.

As the vase had a piece broken from the side, which was lying within it, and was without any foot or cover, it would appear that it had been discovered at some former period; but why it should have been left when every thing else was removed, and how the gold especially had so long escaped, it is hard to determine.

Our unsought good fortune induced us to search many times afterwards, in and about the same spot, but we never found any other object of interest.

One of the most remarkable circumstances attending this vase was its situation, for though it is evident, from its contents that it was used as a cinerary urn, yet it was found placed with an interred body, the niche in which it stood forming part of the grave, as shewn in the diagram.



It is probable that the remains which occupied each, the grave and the vase, were those of persons nearly connected, yet why this one body should have been burnt and all the others interred, at periods of time probably not very distant, has hitherto baffled the conjectures of persons conversant with the usages of antiquity. I am not aware that there exists any other example of the two modes of sepulture being thus united.

Though I had got the vase safe in my possession, which, by the right of my being the finder, became my property, I had still to apprehend that it might be stolen, or maliciously broken, during my residence at Athens. Such dangers, however, it happily escaped, though not without causing me some solicitude and anxiety.

On leaving Athens for Constantinople, I placed it under the

protection of Mr. Gropius, consul for Austria and Sweden, requesting him to send it to England by the first opportunity that might offer. When I returned to Athens from Asia Minor, in the following June, the vase was still in the possession of Mr. Gropius, the opportunities of transport from Athens being very rare. On finally leaving Athens, I took leave of the vase and of several cases of casts, which I had made from the ornamented parts of the temples, without any very sanguine expectation of ever seeing them again. However, as I afterwards found, Mr. Gropius succeeded in sending them safe to Smyrna, whence Mr. Werry, the English consul, whom I had previously apprized of the probability of their coming, sent them to England, and there, on my return in September, 1821, I had the satisfaction to find they had safely arrived six months previously.

The plate represents the vase as lying on the edge of the depository from which it was raised, with its contents partly removed; some fragments of bones, and the golden band, are on the ground. The Acropolis of Athens, Mount Anchesmos, and part of Hymettus, in the distance.—*(See Plate I.)*

Art. VII. *On the Corrections to be applied in Barometrical Mensuration, for the Effects of Atmospheric Vapour, by means of the Hygrometer.* *By* J. F. Daniell, Esq. F. R. S. *and* M. R. I.

Ever since the celebrated and important experiment of Torricelli, the attention of some of the greatest philosophers has been drawn in succession to the interesting problem of the mensuration of heights by means of the barometer. The most laborious experiments have been undertaken for the improvement of the practical part of the operation, and the utmost refinements of mathematical calculation have been employed in the perfecting of its theory. To the former, M. De Luc, General Roy, and Sir George Shuckburgh, have pre-eminently contributed, while the powerful minds of Halley, Newton, Playfair,

and Laplace, have been applied to the latter. But one desideratum in physics has stopped the progress of each at nearly the same point; a desideratum which all have felt, and all in succession have pointed out. I allude to the deficiency of means to measure the quantity and effects of aqueous vapour in the atmosphere. The relation of the air's density and elasticity, the effects of heat upon the relative weights of mercury and air, the diminution of gravity in ascending from the surface of the earth, its variation in different latitudes, and the disturbance of centrifugal force, have been appreciated and allowed for; but all the corrections, except the two first, are exceeded in value by that which has hitherto been only the subject of conjecture, namely, the correction for moisture. Some of the latter calculations have indeed assumed an appearance of considerable accuracy, but while the more important problem remains unsolved, such appearance is merely illusory, and it may fairly be doubted, whether the state of physical science is ever likely to be such, as to render the introduction of the refinements which they exhibit, practically advantageous. The importance, however, of the problem, the solution of which I am now about to attempt, has on the contrary been universally admitted.

M. De Luc, in his valuable and laborious "Researches upon the Modifications of the Atmosphere," thus adverts to the knowledge which it is necessary to obtain of the effects of vapour in the air for the perfecting the mensuration of heights by means of the barometer.

"Voilà donc un nouveau champ ouvert aux expériences. Il s'agit de déterminer quel changement on doit faire à la hauteu trouvée par les logarithmes, quand l'air est plus ou moins chargé de vapeurs qu'un certain point fixe, et de vapeurs échauffées plus ou moins qu'un certain dégré. Il me semble que, pour découvrir cette loi, il faudroit pouvoir joindre l'observation d'un hygromètre comparable à celle du baromètre et thermomètre. Car le point essentiel consiste à connoître s'il y a des vapeurs dans la colonne d'air qui est interceptée par les deux stations, et quelle est leur quantité, puisque, si les

vapeurs qui font baisser le baromètre sont plus élevées que cette colonne, elles ne changent point la loi générale qui sert de fondement au calcul.

"Lorsqu'on aura obtenu ce premier point, il sera facile de connoître par l'expérience, 1°, si les vapeurs influent de la même manière, quel que soit la densité de l'air produite par la pression supérieure, et par consequent, quelle que soit la hauteur du mercure dans la baromètre; 2°, quel rapport il y a entre la quantité des vapeurs exprimée par les dégrés de l'hygromètre, et la diminution d'élasticité de l'air, par une température donnée; ou plus directement, quelle partie proportionnelle il faut déduire de la hauteur trouvée par le calcul; ou ajouter à cette hauteur, pour chaque dégré de l'hygromètre, quand l'air est à cette température; 3°, enfin, quelle modification doit éprouver ce rapport, lorsque la chaleur est plus ou moins grande que le point fixe, auquel la force expansive des vapeurs est égale à celle de l'air.

"Je conviens que tout cela présente bien des soins et des peines au premier coup-d'œil, mais j'ai éprouvé plus d'une fois, que les difficultés connues s'applanissent beaucoup quànd on les affronte avec courage."—Tome iii. p. 288.

General Roy, in commenting upon his experiments upon the different expansions of dry and moist air, for the elucidation of the same subject, says,—

"I am aware it will be alleged that the proportion of moisture admitted into the manometer in these experiments is greater than what can ever take place in nature; and therefore in order to be able to judge of the degrees of expansion, the medium suffers in its more or less dense and more or less moist states, that not only air near the surface of the earth, but likewise that found at the top of some very high mountain, should have been made use of. I grant all this; but on the other hand, it must be remembered that those experiments are very recently finished; that a good hygrometer, (if such can ever be obtained) a great deal of leisure time, and the vicinity of high mountains, were all necessary for the carrying of such a scheme into execution. It is for these reasons, and in hopes that other

people will sooner or later investigate this matter still farther, not only by experiments made on the expansion of air taken at different heights above the level of the sea in middle latitudes, but likewise on that appertaining to the humid and dry regions of the atmosphere towards the equator and poles, that I have been induced to hasten the communication of this paper. In the mean time, having proved, beyond the possibility of doubt, that a wonderful difference doth exist between the elastic force of dry and moist air, I may be allowed hereafter to reason by analogy on the probable effects this will produce in measuring heights by the barometer." Phil. Trans.; vol. lxvii. p. 714.

M. La Place, who has applied the prodigious powers of his science to the perfecting the barometric *formula,* and has availed himself of all the accuracy of the modern method of experiment, was forced to leave the hygrometric state of the air in the catalogue of inevitable errors, contenting himself with an approximate correction.

"Les corrections," says he, "relatives à la latitude, et à la variation de la pesanteur, sont très-petites; mais commes elles sont certaines, il est utile de les employer pour ne laisser subsister dans le calul que les erreurs inèvitables des observations, et celles qui résultent des attractions inconnus des montagnes, *de l'état hygrométrique de l'air, auquel il serait nécessaire d'avoir égard* et enfin de l'hypothèse adoptée sur la loi de la diminution de la chaleur. On tiendrait compte en partie, de l'état hygrométrique de l'air en augmentant un peu le coefficient 0.00375 de $\frac{t+t'}{2}$ dans la formule précédente; car la vapeur aqueuse est plus légère que l'air, et l'accroissement du température en accroit la quantité toutes choses égales d'ailleurs."—*Mécanique Céleste*, Tom. iv. p. 292.

The late lamented Mr. Playfair, in an elaborate paper upon the same subject, published in the Philosophical Transactions of Edinburgh, (Vol. i. 1778.) thus enforces the same argument: "There is another cause of error which, had the effects of it been sufficiently known, ought, no doubt, to have entered into this investigation. Moisture when chemically united to air,

or dissolved in it so as to form part of the same homogeneous and invisible fluid, appears to have a powerful effect to increase the elasticity of the air and its expansion, for every additional degree of heat which it receives. Though the judicious and accurate experiments of General Roy have ascertained this effect of humidity, and have even gone far to determine the law of its operation, yet, for want of a measure of the quantity of it contained at any given time in the air, it is impossible to make any application of this knowledge to the object under our consideration."

Lastly, Mr. Leslie, in an article upon Barometrical Measurements in the *Supplement to the Encyclopædia Britannica*, concludes his detail of corrections with the same acknowledgment: "The humidity of the air also materially affects its elasticity, and the hygrometer should therefore be conjoined with the thermometer in correcting barometrical observations. But nothing satisfactory has yet been done with regard to that subject. The ordinary hygrometers, or rather hygroscopes, are mere toys, and their application to science is altogether hypothetical."

Impressed with the importance of the object so clearly pointed out by a succession of the most able philosophers, I had no sooner succeeded in constructing an instrument which, upon unerring principles would shew the quantity of vapour contained in the air at any given time, than I turned my attention to render it available to the desired purpose. My first ideas and speculations upon the subject were, however, very incorrect. In common with some others, who had not sufficiently considered the subject, I imagined that the elastic force of the vapour was added to that of the permanent gases without producing any change in the latter, leaving the effects of expansion entirely out of consideration. But, without entering into details of difficulties which would only tend unnecessarily to complicate the subject, I shall now endeavour to explain a method of observation and calculation, which, I trust, will be found fully and strictly to solve the important problem of the effects of atmospheric vapour upon barometrical mensuration.

The most simple way of considering the subject, in a general point of view, appears to me to be that which was, I believe, first suggested by Sir George Shuckburgh, (*Phil. Trans.* Vol. lxvii. p. 556.) namely, to make a comparison of the specific gravities of mercury and air at a fixed temperature, and under a given pressure, the foundation of the operation. In this manner we calculate the height of a column of air, compared with any given column of mercury of equal base, supposing it of equal density throughout. The calculation of the gradual diminution of density, which takes place, for equal ascents in the atmosphere, according to a geometrical progression, is made in the usual manner, by means of logarithms. This latter calculation may be deemed invariable under all circumstances; the former includes all the adventitious circumstances, and all the effects of disturbing causes.

The well-known accuracy of MM. Biot and Arago, assisted by the nicety of modern instruments, has determined the relative specific gravities of dry air and mercury at a temperature of 32°, and under a pressure of 30.00 inches to be as 1 to 10.435. The height of a column of air, therefore, of equal density throughout, which would balance a column of mercury of 30 inches under these conditions, would be very nearly 26.090 feet.

Now these proportions may be disturbed in two ways by the operation of heat. In the first place, its expansive power acting upon the mercury may dilate or contract its particles, so that a column of 30 inches, being more or less dense, will require an equipoise of greater or less length, according as its temperature is below or above the standard at 32°. This effect has been most minutely appreciated, and its correction is applied with the utmost ease and precision. In the second place, the power of heat acting upon the air occasions a much more important dilatation or contraction of its parts, and gives rise to much greater differences in the height of the equiponderant column. The expansion of air has been determined with precision by the experiments of M. Gay Lussac, and from them we infer, with confidence, that it increases or diminishes $\frac{1}{480}$ part for every addition or subtraction of 1° of heat.

In this situation, therefore, the operation stands: the column of mercury which is the measure applied, is rendered an invariable standard of comparison, by being brought by an easy calculation to a known density; the altitude measured is then in proportion to the specific gravity of the air.

But heat is not the only agent which alters the specific gravity of air, the admixture of aqueous vapour, it is well known, also produces very important changes. It did not, as I have shewn, escape the observation of General Roy, that air, in contact with water, expanded much more than dry air, and from well-conducted experiments he ascertained that the expansion was greater, for equal increments, as the temperature rose. From the mean results which he obtained, the following increasing rates of expansion were derived:

1000 parts of air in contact with water, and under a pressure of 32.18 inchesexpanded for each degree,

From	0°	to	32°	2.22799
	32	,,	52	2.58800
	52	,,	72	2.97228
	72	,,	92	3.63194
	92	,,	112	4.91072
	112	,,	132	6.86550
	132	,,	152	9.89494
	152	,,	172	12.04087
	172	,,	192	17.88344
	192	,,	212	19.22470

Hence we have the progressive scale of expansion as follows:

0°	100000
32	107129
52	112305
72	118250
92	125514
112	135375
132	149106
152	168896
172	192978
192	228744
212	267194

To compare this with the results of modern experiments, we must have recourse to calculation. The accurate experiments of Mr. Dalton will furnish the data. A volume of dry air of given elasticity being mixed with vapour, also of known elasticity, will have its volume increased in proportion to the elasticity of the mixture. Thus, a cubic foot of air of the temperature of 212° which would support a column of mercury of 30 inches, being mixed with a cubic foot of vapour of 212° also of the elasticity of 30 inches, would occupy a space of two cubic feet.

For 30 inches : 60 inches : : 1 : 2.

Now from the experiments of Mr. Dalton, we are acquainted with the force of vapour for every degree of a very long range of the thermometric scale, and if we assume the volume of dry air at 0° temperature, and 30 inches' pressure, as 1.00000, the calculation is sufficiently simple. For example, let it be required to know the expansion which would take place in air in contact with water by a rise from 0° to 32°. The force of vapour of that temperature, according to Mr. Dalton's table, is 0.200 inches, therefore

$$30.000 : 30.200 :: 1.00000 : 1.00666.$$

This is the expansion which would arise from vapour only: to this we must add the expansion which would take place from the addition of heat, *viz.* $\frac{1}{480}$th part for every degree.

Now $.002083 \times 32° = .066656$, which added to 1.00666, makes the total expansion 1.07332.

In this manner I have calculated the following scale, corresponding with General Roy's:

0	100000
32	107332
52	112169
72	117565
92	123966
112	132266
132	142832
152	157699
172	178266
192	206199
212	244166

This, upon the whole, exhibits a very surprising and gratifying degree of accordance, especially in the upper part, and considering the disadvantages under which the General laboured, does infinite honour to his accuracy and perseverance. Little more was wanting to have enabled him to complete the solution of the problem, towards which he contributed so many steps, than an instrument to measure the quantity of the agent, the effects of which he so well appreciated.

But the expansion which vapour causes in air, is not precisely similar to that occasioned by heat, for while it dilates its parts, it adds its own weight to the mixture. Let it be required to know the specific gravity of air at 32° saturated with vapour, compared with dry air at the same temperature. Call the latter 1.00000. The quantity of expansion will be .00666, which deducted from 1.00000 leaves .99334. Now the weight of a cubic foot of air, under the conditions above named, is 563.414 grains, and the weight of a cubic foot of vapour at 32° is about 2.317 grains, which the former being 1.00000, will be nearly .00411, and which added to the .99334 before obtained, will give .99745 for the specific gravity sought.

Upon this principle I have constructed the following table, by means of which, the specific gravity of any mixture of atmospheric air and aqueous vapour from 0° to 90° may readily be found with sufficient precision. I have made air under a pressure of 30 inches of mercury, and at the temperature of 32°, the standard of comparison. The first column contains the degrees of Fahrenheit's thermometer—the second shews the quantity due to each degree of heat, to be subtracted or added according as the temperature is above or below the standard—the third exhibits the expansion of volume occasioned by vapour of the respective degrees of elasticity appropriate to the several degrees of heat, and is always to be subtracted—the fourth is the correction to be applied for the weight of the vapour, and is constantly to be added,—and the fifth is the correct specific gravity, supposing the air saturated with moisture at the given temperature.

Table for finding the Specific Gravity of any Mixture of Air and Aqueous Vapour, from 0° *to* 90° *Dry Air at* 32° *Temperature, and* 30 *Inches Pressure, being* 1.00000.

Temperature.	Alteration of Volume from Heat.	Alteration of Volume from Vapour.	Increase of Density from Weight of Vapour.	Correct Specific Gravity of Saturated Air.
0	+ .06666	− .00213	+ .00140	1.06593
1	+ .06458	− .00220	+ .00144	1.06382
2	+ .06249	− .00226	+ .00148	1.06171
3	+ .06041	− .00236	+ .00154	1.05959
4	+ .05833	− .00246	+ .00160	1.05747
5	+ .05624	− .00253	+ .00164	1.05535
6	+ .05416	− .00263	+ .00170	1.05323
7	+ .05208	− .00273	+ .00176	1.05111
8	+ .04999	− .00283	+ .00183	1.04899
9	+ .04791	− .00290	+ .00187	1.04688
10	+ .04583	− .00300	+ .00193	1.04476
11	+ .04374	− .00310	+ .00199	1.04263
12	+ .04166	− .00320	+ .00205	1.04051
13	+ .03958	− .00333	+ .00213	1.03838
14	+ .03749	− .00346	+ .00221	1.03624
15	+ .03541	− .00360	+ .00229	1.03410
16	+ .03333	− .00373	+ .00237	1.03197
17	+ .03124	− .00386	+ .00243	1.02981
18	+ .02916	− .00400	+ .00253	1.02769
19	+ .02708	− .00413	+ .00261	1.02556
20	+ .02475	− .00430	+ .00271	1.02316
21	+ .02291	− .00446	+ .00281	1.02126
22	+ .02083	− .00463	+ .00291	1.01911
23	+ .01874	− .00480	+ .00301	1.01695
24	+ .01666	− .00500	+ .00312	1.01478
25	+ .01458	− .00520	+ .00324	1.01262
26	+ .01249	− .00540	+ .00336	1.01045
27	+ .01041	− .00560	+ .00347	1.00828
28	+ .00833	− .00580	+ .00360	1.00613
29	+ .00624	− .00600	+ .00372	1.00396
30	+ .00416	− .00620	+ .00384	1.00180
31	+ .00208	− .00643	+ .00398	0.99963
32	.00000	− .00666	+ .00411	0.99745
33	− .00208	− .00690	+ .00424	0.99526
34	− .00416	− .00713	+ .00436	0.99307
35	− .00624	− .00736	+ .00448	0.99088
36	− .00833	− .00763	+ .00464	0.98868
37	− .01041	− .00790	+ .00480	0.98649
38	− .01249	− .00816	+ .00495	0.98430
39	− .01458	− .00846	+ .00513	0.98209
40	− .01666	− .00876	+ .00531	0.97989
41	− .01874	− .00910	+ .00553	0.97769
42	− .02083	− .00943	+ .00573	0.97547
43	− .02291	− .00980	+ .00596	0.97325

Temperature.	Alteration of Volume from Heat.	Alteration of Volume from Vapour.	Increase of density from Weight of Vapour.	Correct Specific Gravity of Saturated Air.
44	— .02475	— .01016	+ .00616	0.97125
45	— .02708	— .01053	+ .00637	0.96876
46	— .02916	— .01093	+ .00661	0.96652
47	— .03124	— .01130	+ .00681	0.96427
48	— .03333	— .01170	+ .00704	0.96201
49	— .03541	— .01210	+ .00731	0.95980
50	— .03749	— .01250	+ .00753	0.95758
51	— .03958	— .01293	+ .00775	0.95524
52	— .04166	— .01336	+ .00797	0.95295
53	— .04374	— .01383	+ .00823	0.95066
54	— .04583	— .01430	+ .00848	0.94840
55	— .04791	— .01476	+ .00872	0.94605
56	— .04999	— .01526	+ .00899	0.94374
57	— .05208	— .01580	+ .00929	0.94141
58	— .05416	— .01633	+ .00958	0.93809
59	— .05624	— .01690	+ .00990	0.93676
60	— .05833	— .01746	+ .01020	0.93440
61	— .06041	— .01806	+ .01054	0.93207
62	— .06249	— .01866	+ .01088	0.92973
63	— .06458	— .01926	+ .01121	0.92737
64	— .06666	— .01990	+ .01157	0.92500
65	— .06874	— .02053	+ .01192	0.92265
66	— .07083	— .02116	+ .01226	0.92027
67	— .07291	— .02183	+ .01264	0.91790
68	— .07499	— .02253	+ .01304	0.91552
69	— .07708	— .02326	+ .01344	0.91310
70	— .07916	— .02403	+ .01385	0.91066
71	— .08124	— .02483	+ .01425	0.90818
72	— .08333	— .02566	+ .01468	0.90569
73	— .08541	— .02653	+ .01514	0.90320
74	— .08749	— .02743	+ .01563	0.90071
75	— .08938	— .02836	+ .01613	0.89839
76	— .09166	— .02933	+ .01665	0.89566
77	— .09374	— .03033	+ .01719	0.89312
78	— .09583	— .03133	+ .01773	0.89057
79	— .09791	— .03236	+ .01829	0.88802
80	— .09999	— .03333	+ .01879	0.88547
81	— .10208	— .03466	+ .01962	0.88288
82	— .10416	— .03566	+ .02012	0.88030
83	— .10624	— .03666	+ .02062	0.87772
84	— .10833	— .03800	+ .02141	0.87508
85	— .11041	— .03900	+ .02184	0.87243
86	— .11249	— .04033	+ .02257	0.86975
87	— .11458	— .04133	+ .02297	0.86706
88	— .11666	— .04266	+ .02370	0.86438
89	— .11874	— .04430	+ .02474	0.86170
90	— .12083	— .04533	+ .02511	0.85895

To find the specific gravity of any mixture of air and aqueous vapour by means of this table, we must proceed as follows:

Note the temperature and the point of condensation by means of the hygrometer: if they coincide, that is to say, if the air be in a state of saturation, we shall find the specific gravity required in the fifth column opposite to the proper degree of heat in the first column. If the point of condensation be below the temperature, we must look for the correction to be applied separately for the heat in the second column. The quantity to be subtracted for the vapour of the proper degree must be sought for in the third column, and this must be applied *minus* the quantity due to its weight, which stands beside it in the fourth.

For example: if we wish to know the specific gravity of a mixture of air and vapour of the temperature of 60°, and of which the dew point is 40°, we find in the second column opposite to 60° the number .05833, which, deducted from 1.00000, leaves .94167. In the second column, opposite to 40°, we have .00876, and beside it in the third .00531. Now .00876 —.00531 = .00345, which subtracted from .94167, leaves .93822 as the number sought for.

The application of this table to barometrical mensurations is sufficiently simple. For this purpose, with the usual operations at the upper and lower stations, must be combined simultaneous observations of the dew point by means of the hygrometer, and the approximate height may be corrected by the specific gravity of the atmosphere so obtained. As the specific gravity of the air at the time of the experiment is to 100000 the standard, so will the approximate height be to the real height.

I have not had the advantage of an opportunity of making any trials practically to prove the superior accuracy of this correction, but the theoretical reasons in its favour are conclusive, and to these I may add two or three particulars derived from experience.

General Roy, from his own experiments, as well as from a careful review of those of De Luc, fixed the medium expansion of air for 1° = .00245, and Sir George Shuckburgh assigned

.00243 as the mean. Now these estimates derived from barometrical experiments, made at different temperatures and compared with known heights, must have included also the expansion due to the mean quantity of vapour; and upon referring to the table we shall find that the medium of the two combined effects is exactly .00243 for every degree of temperature: for 0.85895 deducted from 100000, leaves 0.14105, which divided by 58, the total number of degrees from 32° to 90 gives .00243.

General Roy, again, has fixed the point of temperature at which the specific gravity of mercury to the atmosphere is 10.435 at 32° the average of his experiments, making it, however, a little lower. Sir George Shuckburgh places it at 31¼. It is worthy of remark that the point which approaches the nearest by the table to exact coincidence is 31°, for at 32° the effects of temperature being null, a fall of one degree is necessary to neutralize the expansion of the vapour.

But it may be asked, Does vapour, thus acting upon the atmosphere, and changing the degree of its absolute elasticity, act in such a manner as to change it equally at all heights, "so that though different at different times, it shall always at any one time be the same at all different heights?"

This requires a little consideration. Vapour, uninfluenced by circumstances of external temperature, would, I conceive, be subject to the same law as the permanent gases of the atmosphere; it would decrease, namely, in density, in a geometrical progression for equal ascents. By the application of logarithms, we may therefore ascertain what the decrease of density would be for any given height, and consequently its constituent temperature.

Let it be required to know the decrease of density which would take place in an atmosphere of vapour of the elasticity of 0.200 inches, and whose constituent temperature is therefore 32° in an ascent of 10,000 feet; we must first find the height of an homogeneous atmosphere of such vapour equivalent to 0.200 inches of mercury.

But its specific gravity, compared to dry air, is as 2.317 to 557.800, therefore 2.317 : 557.800 :: 10435 : 2,512,146; then 2,512,146 × 0.2 ins. = 502429 ins. = 41.869 feet.

Height of homogeneous Vapour.		Feet.		Modulus of Logarithms.		Diff. of Logs. of Densities.
41869	:	10,000	::	.4342945	:	.1037198

Density of vapour at 32°		
Log. of .200	=	.3010300
	—	.1037198
	=	.1973102 Log. of .157

which is the elasticity of vapour at 25°.

To select another example from the other extremity of the scale, let us take vapour at 212°, and 30 ins. elasticity. The height of the homogeneous atmosphere we shall find 56,567 feet.

56,567 : 10,000 feet :: .4342945 : .0767752

Density of vapour at 212°		
Log. of 30.00	=	.4771212
	—	.0767752
	=	.4003460 = 25.14

the elastic force of vapour at $203\frac{1}{2}$.

In this manner I have found, by various trials and for different altitudes, that the elastic force of vapour would decrease in such a proportion as to lose 1° of constituent temperature for every 1250 feet; or, in other words we may say, more correctly, that it is a necessary condition of an atmosphere of pure vapour, that its constituent temperature increase from above downwards 1° for every 1250 feet. But this can never be exactly the case with vapour in the state of mixture as it exists in our atmosphere. The nearest approximation to it is when its constituent temperature is very considerably below the general temperature; as, for example, when the point of precipitation is at 40°, and the heat of the air 70°. The heat of the air, decreasing 1° for about every 290 feet of elevation, would not attain the precipitating point at a less height than 8700 feet. Within this range, then, the vapour would be left to its own law of density, but still modified by the excess of heat. In the case of complete saturation, however, it is evident that

the state of the vapour must be regulated by the state of the general temperature, and its density will be governed by its more rapid decrease. It is sufficient for our present purpose to have ascertained that in both cases the constituent temperature of vapour tends to a gradual though very different decrease by equal differences, as we ascend through equal spaces, and the general law of nature favours equality of change throughout the whole of any given column of the atmosphere.

To this, as well as to the general law of the diminution of heat in ascending in the atmosphere, we may apply the observation of Professor Playfair. "This law is subject to certain anomalies, both annual and diurnal; and those intermixed with other accidental irregularities, which it would be difficult, perhaps impossible, to ascertain. All that can be said of it is, that it is the law which nature tends to observe, and that the sum of the deviations from it on the one side is probably equal to the sum of those on the other."—Edinburgh Phil. Trans.; vol. i. (1788.)

It will be matter for future investigation, to ascertain more accurately in what manner the two principles modify each other; for there can be little doubt but that the presence of vapour influences the gradations of heat as much as the latter obviously regulates the former.

I shall now conclude this paper by supposing a case in which all the proper observations have been made for the purpose of shewing more distinctly the manner in which I propose to apply the table of correction.

Barom. at lower station	29.528		Temp. of mercury,	58°
.... upper station	28.161			51¼
Correct for density of mercury	29.528 .084			
		29.444	Log.	.4689378
Do. Do.	28.161 − .060			
		28.101	Log.	.4487063
Approximate height in fathoms				202.315 × 6
Do. Do. in feet				1213.890

Temp. of air at lower station	55	Dew point	40
Do. upper station	$51\frac{1}{2}$	Do.	38
Mean	$53\frac{1}{4}$		

Expansion of air, per Table, at 53° .04374

$$\begin{array}{r} 1.00000 \\ -\ 04374 \\ \hline .95626 \end{array}$$ Specific gravity of air corrected for temperature.

Expansion of air for vapour at 40°, per Table,	.00876	
– Increase of density for ditto	00531	
		.00345
Expansion of air for vapour, at 38°	.00816	
– Increase of density	.00495	
		.00321
		.00666
	Mean	.00333

$$\begin{array}{r} .95626 \\ -\ .00333 \\ \hline .95293 \end{array}$$ correct specific gravity of atmosphere. Then

$$.95293 : 1.00000 :: \underset{\text{Approx. height.}}{1214} : \underset{\text{Correct height.}}{1273}$$

Art. VIII. *Account of a Steam-Engine Indicator.*

[In a Letter to the Editor.]

Glasgow, 14*th Feb.* 1822.

This instrument which has been long known to engineers, though I believe never publicly described, originated with our illustrious countryman, the late Mr. Watt. That which was employed in our works was made from a description of the instrument given to us by Mr. Field, of London, and was applied with the view of ascertaining in what part the extraordi-

nary friction existed, which had for many months previous prevented the engine from attaining its proper speed.

In the first experiments we found, when the whole work was in motion at the usual velocity, that the average pressure upon each square inch of the piston amounted to 11.7 lbs., or about 1-3d more than it ought to have been; and that at this time the engine, which is of forty-five horse power, made at Soho, exerted a power, including its own friction, of more than 70 horses. Finding, in repeated trials with detached parts of the work, that the same extraordinary absorption pervaded every department, we became convinced that the effect was produced by some generally existing cause, and naturally directed our attention to the quality of the oil which was in use throughout the work. At one time we were in the habit of using that which is known under the name of neat-foot, but owing to its scarcity, and the extreme cheapness of rape oil, we began, about two years ago, to mix with the former a small portion of the latter. No apparent difference being discovered by the workmen, this proportion was gradually increased, until the stock of neat-foot oil being at last consumed, the rape oil became the sole anti-attrition. And so gradually had the change been effected, that the workmen even to the last denied the existence of any unusual friction, and attributed the want of speed solely to some defect in the engine, which daily exhibited stronger symptoms of being overloaded. At length the use of the rape oil was suspended, and spermaceti substituted; and in twenty-four hours the average pressure was reduced to 9.5 lbs.; in a week after it had fallen to 9.1. At this time a mixture of one-third sperm. and two-thirds rape oil was given out to the workmen, and the friction, after the first day, gradually increased, until at the end of a fortnight the average pressure became 11.1 lbs. A return to the pure sperm oil again reduced the pressure to 9.5. Subsequent observations have given from 8.7 to 9.6, but the inconstant motion of the machinery in a cotton mill must at all times produce such a difference.

It has long been a maxim with us, and we believe is pretty generally acted upon by those at the head of manufacturing establishments, to receive and adopt the reports of our workmen upon things of which we think they have, from their different occupations, an opportunity of forming a just opinion: the present instance shews, however, that even in a situation where their daily expenditure of exertion was increased at least 15 or 20 per cent., they were unconscious of the change, and would only be convinced of its existence by a rapid transition from one extreme to the other. The results, therefore, of experience, or rather the opinions which men form when, from the nature of the subject, their observations are confined to effects alone, ought to be received with care, and acted upon with caution; and it is only upon the unbiassed results exhibited to our actual observation, through the medium of inanimate matter, acting on known principles, that implicit confidence ought to be placed. The indicator is an instrument of this kind; it exhibits to our view the successive changes of pressure which take place in a steam-engine cylinder during each stroke; and by also marking the duration of each particular pressure, it affords, with an elegant simplicity, a very near and correct approximation to the power exerted. The results which it yields are so *tangible*, and in many situations so important and instructive to those who have the distribution and application of the power derived from steam-engines, that we think it only requires to be more generally known and understood, to be oftener applied.

H. H. Jun.

Glasgow, 4th Feb. 1822.

Description of a Steam-Engine Indicator.—Plate II.

A—Steam-engine cylinder cover.

B *Stop-cock*—usually made to answer the seat of the grease-cock.

C *Indicator cylinder*—about $1\frac{3}{4}$ inches diameter, and 8 inches long, open at top, and screwed at bottom upon the stop-cock B.

D *A flat pillar*—screwed to the side of the cylinder C, and supporting the frame EE.

F *The piston*—fitted so as to work easily up and down, and to be, at the same time, air-tight.

EE *A frame*—12 inches, by 7 inside, the under and upper rail grooved to retain the sliding-board K.

G *The piston rod*—about 5-8ths diameter, and 16 inches long.

H *A guide*—screwed to the pillar D, at 6 inches above the top of the small cylinder, and through which the piston-rod passes.

I *A spiral spring*—attached to the piston at F, and the guide at H. It should be about 7 inches long when at rest, and of such a strength as to allow the piston to descend nearly to the bottom of the cylinder, when it is loaded with a weight equal to 14 lbs. upon every square inch of its area. It should also admit of being compressed about $1\frac{1}{2}$ inches.

K *A small board*—about 7 inches square, sliding in grooves in the upper and under rails of the frame EE.

L *A small brass socket*—which may be fixed at any height upon the piston-rod, by the tightening screw M. It carries in the other end a short pencil, with a weak spring to push it forward against the surface of the sliding-board.

N *A weight*—attached by a cord to the sliding-board K.

O—Any convenient part of the parallel motion, traversing a space of about $4\frac{1}{2}$ inches during each half stroke of the engine.

From this description the principle on which the instrument acts will be evident.

By opening the stop-cock B, a direct communication is made between the interior of the large and small cylinders, and the density of the steam in the indicator becomes the same as in the steam-engine cylinder above the piston. When this density is less than that under the atmospheric pressure, the indicator piston will sink, when it is greater the piston will rise; but the spiral spring, which, if carefully made, stretches through equal distances with equal weights, restrains the motion of the

piston; and, by the distance to which it allows it to move from its state of rest, indicates the pressure it is undergoing. During each stroke of the engine, therefore, the indicator piston will rise at the instant the upper steam valve opens, and during the descent of the large piston will maintain a height proportioned to the density of the steam in the cylinder. When the eduction valve opens it will sink, and by the rapidity of its descent, and the distance to which it falls, denote the quality of the vacuum. If, during this perpendicularly alternating motion of the small piston, the sliding-board be made to perform its reciprocating and horizontal course, the pencil, in the socket L, will trace upon the board, or upon a piece of paper applied to its surface, a figure, something like P Q R S; of which figure, the part P Q is drawn during the descent of the large piston. At Q, the condensation taking place, the atmospheric pressure acts upon the piston indicator, and makes it descend until the tension of the spring counteracts the force of the pressure. Meanwhile the engine begins to perform the up-stroke, and, as the board traverses, produces the line R S. When the engine piston arrives at the top, the admission of the steam destroys the vacuum that existed below the indicator piston, and allows the spring to raise the latter until the equilibrium is restored. It consequently follows that the distance between the line P Q, and the line R S, will be greater in proportion to the difference between the pressure in the cylinder during the existence of the vacuum and the pressure of the steam, and the curve Q R be more acute in proportion to the rapidity with which the vacuum is formed. If this distance be measured in eight or ten places, and an average taken, then a simple proportion gives the pressure upon each square inch of the piston. Let $a =$ area of indicator piston,—b, any weight applied experimentally to that piston,—d, the distance to which it falls with that weight,—and let e be the average distance taken from a diagram, and f the average pressure in pounds upon the steam-engine piston during the formation of that diagram. Then, as d to $\frac{b}{a}$ so is e to f, or $\frac{be}{da} = f$. And

as for every individual instrument, a b d are constant quantities; then $\frac{b}{da} = x$, a constant number by which to multiply the average distance obtained from a diagram, for the average pressure in pounds upon each square inch of the steam piston. Mr. Hutton, of the Anderston Foundry, near this city, now makes these instruments upon a very convenient plan, and as he attaches to each the constant multiplier by which the result is obtained, their application is rendered perfectly simple.

Art. IX. *A Review of some of the General Principles of Physiology, with the practical Inferences to which they have led. By* A. P. W. Philip, M.D., F.R.S., Edinb.

It is not my intention in the following observations, to attempt a general view of the state of our knowledge respecting the vital powers of the animal system, but to confine myself to those parts of the subject to which my attention has been particularly directed, embracing what is known respecting the nature of these powers, and their relation to each other. This is far from comprehending the whole of the subject, but forms the basis on which the whole must rest.

As there are few of the general principles of physiology which have not given rise to much difference of opinion, it is only by a tedious investigation, for which the members of our profession seldom find leisure, that we can arrive at the truth; yet every one will admit that a knowledge of these principles is always useful and in many instances essential in the treatment of disease. I cannot help thinking, therefore, that I shall perform an acceptable task if I lay before them a simple statement of the facts we possess on the subject, with references to the means by which they have been ascertained; so that those who wish to recur to the original sources may do so without trouble, and with such a general knowledge of the subject, as will enable them to form a correct judgment of its several parts.

I have another motive, however, for undertaking the task I have imposed on myself. It appears to me that when the whole of the facts are brought together, we shall be led to inferences of considerable importance in the animal economy, which a more partial view cannot afford.

The animal differs from the vegetable world in possessing sensation and volition. These functions we shall find are essential to the continuance of life, at least in the higher classes of animals, and consequently as far as they are so, belong to the department of the physiologist, the other functions of the mind which form no part of the powers by which life is preserved, belonging exclusively to that of the moralist. By the term sensorial powers, therefore, which I shall employ for the sake of brevity, I mean sensation and volition alone.

Both the animal and vegetable world differ from inanimate matter, in affording a peculiar class of results when impressed by other agents, whether chemical or mechanical. The quality on which the peculiarity of these results depends, has been termed the vital principle. Whether this principle be something superadded to bodies, or only a peculiar arrangement of their constituent parts, we have no means of ascertaining. The fact is, that it bestows on matter certain properties. It is essential that its name should convey this fact, and no more.

The phenomena of the vital principle have been observed and arranged with more care in the animal, than the vegetable world. Of the former alone, notwithstanding the intimate connexion of the subjects, I propose to treat here. They may be divided into two classes, those of the muscular, and those of the nervous system.

I shall begin with the muscular system, because its function is the simplest. It consists merely in a contractile power, by which, in consequence of the impression of certain agents, the extremities of the muscle are made to approach each other, its firmness and transverse diameter being increased in a degree which bears a certain proportion to the approximation of its extremities.

Previous to the time of Haller, it had been universally sup-

posed that the power of the muscular fibre was derived from the nervous system, which was regarded as the general source of power in the animal body; and, many circumstances seem at first view to countenance this opinion. When the nerves are divided, the muscles of voluntary motion can no longer be excited by the will, and when the power of the brain and spinal marrow is destroyed, that of the muscles never long survives. In like manner if a limb be removed from the rest of the body, all its powers soon cease.

It was taught by Haller, however, that the power of muscles resides in themselves, and is only lost when the influence of the nervous system is withdrawn, in consequence of the failure of other powers, and that this influence acts merely as a stimulus to the muscles, and that only with respect to those of voluntary motion, those of involuntary motion being excited by other means. He thus explains phenomena which appeared inexplicable on the suppositions of his predecessors.

I shall here give a translation, a little abridged, of what is so well said on the state of our knowledge of this subject in the report of the Royal Academy of Sciences, on the experiments of M. Le Gallois.

"The theories of Boerhaave and Stahl reigned almost alone, when, in 1752, Haller published his experiments on irritability. These experiments and those of his followers tend to prove, that the contractile power belongs essentially to the muscular fibre. That property which Haller sometimes speaks of under the name of *vis insita*, sometimes after Glisson under that of irritability, is the source of all the motions which take place in the animal, but it cannot produce them except some cause, some stimulus determines it to act. Thus, all muscular motion implies two things, the irritability which produces the contraction of the muscle, and the stimulus which determines the irritability to act. The irritability is everywhere the same. It only varies in intensity in the different muscles, but it does not obey the same stimuli in all the muscles. The nervous power is the natural stimulus to all those which are under the

influence of the will, and it is by exciting or suspending the action of that power on the irritability of such or such muscles, that the will causes any particular part to act, or be at rest. It is not thus with the muscles of involuntary motion. These are affected by stimuli of different kinds which are appropriated to their different functions, and altogether different from the nervous power. It is the blood which is the natural stimulus of the irritability of the heart, alimentary substances of that of the intestinal canal, *&c.*

We easily deduce from these principles the explanation of the leading circumstances which we observe in the motions of the heart. Thus, its motions are involuntary, because they are independent of the nervous system. They take place without interruption during life, because the irritability which produces them belongs essentially to the fibres of the heart, and the blood which excites them is constantly supplied to this organ by the veins as it is carried off by the arteries. The systole and diastole succeed each other alternately and regularly, because the stimulus of the blood always occasions the former both in the auricles and ventricles, and the systole itself, by expelling the stimulus, occasions the diastole, which renews the systole by allowing access to new blood.

Such is a summary view of the celebrated Hallerian theory of irritability. That theory was not contrived in the closet, like the others of which we have spoken; it was founded, as we have said, on experiments made by Haller and the most distinguished of his scholars. The inferences from these experiments, which were repeated throughout Europe, found almost every where supporters, but they found also some opponents of the greatest reputation. The principal cause of this difference of opinion, and that respecting which authors have not yet been able to come to any agreement, is the question whether the motions of the heart are really independent of the nervous system.

We may reduce to three heads the facts by which the school of Haller has supported the affirmative. 1. If we interrupt all communication between the heart and the brain, the only supposed source of nervous power, by dividing the nerves which

go to the heart and the spinal marrow in the neck, or even by decapitation, the motions of the heart continue as before. 2. If we cut out the heart of a living animal it continues to beat, and sometimes for a long time. 3. We always produce convulsions even for some time after death in the muscles of voluntary motion by irritating their nerves, either mechanically, or in any other way. On the contrary, the irritation of the cardiac nerves occasions no change in the motions of the heart, nor recalls them when they have ceased. The same observation is true of the medulla oblongata and spinal marrow, the irritation of which occasions strong general convulsions, but produces no effect upon the heart.

These facts are correct, except, perhaps, those of the third head. In admitting them, the adversaries of irritability have asked, why, if the nervous power have no action on the heart is this organ supplied with nerves? and why is it so evidently subjected to the influence of the passions? Haller never gave any satisfactory explanation of these objections, but every thing proves that he felt all their force. When we read with attention all that he has said of the motions of the heart, in his Dissertations on Irritability *, and, above all, in his great work on Physiology †, we are struck with the contradictions which we meet with in them, and which makes the perusal of them fatiguing.

Through all of them his great object is to prove, that the motions of the heart are independent of the nervous system, yet he seems to admit, in several places, that the nerves possess an influence over the heart ‡. These contradictions, with which several justly-celebrated writers have reproached him, amongst others, M. M. Prochaska §, Behrends ||, Ernest Platner ¶, &c., proceed evidently from his not being able to recon-

* *Mémoires sur la Nature Sensible et Irritable des Parties*, &c. Lausanne, 1756. *Opera Minora*, tom. i.

† *Element. Physiol.*, lib. iv. sect. 5, et lib. xi. sect. 3.

‡ *Ibid.* lib. 4. sect. 5, p. 493, et passim.

§ *Opera Minora*, Viennæ, 1800, tom. II., p. 90.

|| Vol. III.; p. 4, of the Collection of Ludwig, entitled *Scriptores Neurolog. Minores Selecti*, Lipsiæ, 1791-5, four vols. 4to.

¶ *Ibid.*, vol. II., p. 266.

cile the results of his experiments with the influence of the nervous power over the motions of the heart, and in rejecting this influence, finding it impossible to explain the use of the cardiac nerves and the effect of the passions on the heart. Here is the great difficulty in the controversy of which we speak. Those who, like Fontana, formally reject all intervention of the nervous influence, have been forced to admit that the nerves, destined to convey to every other part life, feeling, and motion, have no known use in the heart *.

Such consequences evidently disclose the insufficiency of the theory of Haller, and several of his followers have acknowledged the necessity of some modification of it, and admit the nervous power to be one of the principles on which irritability depends. They are thus enabled to assign a use to the nerves of the heart, and to explain the influence of the passions on this organ. But when they have attempted to explain why the interruption of all communication between the brain and the heart does not stop the motions of the latter, they have been obliged to abandon the generally-received opinion, which regards the brain as the only centre and source of nervous power; and admit, without any direct proofs, that that power is generated throughout the whole extent of the nervous system, even in the smallest nerves; and that it can exist for a certain time in the nerves of any part, independently of the brain. Among the authors of this opinion the learned Professor Prochaska is one of those who has given the best account of it †. But when he applies it to the motions of the heart, and attempts to explain why they are independent of the will, and yet influenced by the passions, his opinion appears undecided. He has recourse to the ganglions, and hesitates what functions to ascribe to them. Sometimes he considers them as knots, as

* *Mémoires sur les Parties Sensibl. et Irritab.*, tom. III., p. 234. See also Caldani, *ibid.* p. 471; et *le Traité sur le Venin de la Vipère*, tom. II., p. 169—171.

† *Commentatio de Functionibus Systematis Nervosi*, published in the third fasciculus of the *Annotationes Academ.* of this writer, and reprinted at Vienna in his *Opera Minora*, in 1800.

ligatures, so tight as to intercept all communication between the heart and *sensorium commune* in the calm and peaceful state of the system, but not sufficient to prevent the sensorium acting more or less powerfully on the heart in the agitation of the passions *. Sometimes he seems to believe that the interception is complete and constant, and that it is by the nerves of the eighth pair that the passions affect the heart †; and he seems to adopt the opinion of Winslow‡, renewed by Winkel§, Johnstone||, Unzer¶, Lecat **, Peffinger ††, &c., that the ganglions are so many small brains. He admits, at the same time, that the nerves of feeling are distinct from those of motion; so that the heart cannot contract, except when the impression of the stimulus on its cavities is transmitted to the ganglions by the nerves of feeling, and reflected on its fibres by the nerves of motion‡‡. But, besides, that this opinion, even by the author's confession, is only a conjecture, it supposes, on the one hand, that the circulation would continue after the destruction of the spinal marrow; and, on the other, that the heart would cease to beat at the moment when its communication with the ganglions and the plexuses is interrupted. Now, both these suppositions are contradicted by facts.

These fruitless attempts to modify the theory of irritability by the intervention of the nervous power, have only increased the zeal of some authors to maintain that theory in its original purity; and as the use of the nerves of the heart was among the most embarrassing objections to it, M. Sœmmering, one of the most profound anatomists of Germany, and Behrends, one of his most distinguished scholars, maintained, in 1792, that

* *Opera Minora*, tom. II., p. 165. † *Ibid.*, p. 167.

† *Exposit. Anatom. Traité des Nerfs*, § 364.

‡ *Nov. Inflam. Theoria*, Vienn. 1767, cap. 5, p. 154.

§ *Essay on the Use of the Ganglions*, 1771.

|| Unzer, quoted by Prochaska, *Oper. Minora*, tom. iii. p. 169.

¶ *Traité de l'Existence de la Nature et des Propriétés du Fluide Nerveux.* Berlin, 1765, p. 225.

** *De Structura Nervorum.* Argentorati, 1782. Sect. 1. § 34, inserted in the Collection of Ludwig, vol. i.

†† *Opera Minora*, tom. II., p. 169.

the heart has no nerves, and that all those which appear to enter it are expended on the coats of the coronary arteries without the fibres of the heart receiving a single thread *, an opinion which far from removing all the difficulties only renders the influence of the passions on the motions of the heart more inexplicable. These two authors maintain that the cardiac nerves support and increase the irritability of the coronary arteries; but the existence of irritability in the arteries is still doubtful †, and were it demonstrated it would be very strange if irritability depended on the nervous influence in the arteries, and in the heart, the most irritable of all organs, it were wholly independent of this influence.

M. Scarpa in his excellent work ‡ on the nerves of the heart, proves that they are as numerous, and distributed in the same way as in other muscles, The opinion of this author, respecting the power of the heart, is as much at variance with facts as the opinions we have considered; but he makes a very important remark, that the insensibility of the heart, of which so much has been said, and which has been regarded as a demonstrative proof that the motions of the heart do not depend on the nerves, proves only that the nerves of the heart are not of the same kind with those of the muscles of voluntary motion, and that the nervous power does not in them obey the same laws §.

This did not prevent Bichat || from denying that the nervous power has any share in the motions of the heart. This writer

* Behrends' *Dissertatio qua Demonstrator Cor Nervis carere.* Moguntiæ, 1792, inserted in the third volume of the Collection of Ludwig.

† This doubt has since been removed by experiments, which demonstrate the excitability both of the larger arteries and the capillaries. The reader will find an account of many experiments of this kind in the Introduction to my Treatise on Symptomatic Fevers, and Dr. Hastings's Treatise on Inflammation of the Mucous Membrane of the Lungs.

‡ *Tab. Murolog. ad illust. Hist. Anat. Cardiacorum Nervorum*, &c., Ticini, 1794. § *Ib.* § 20.

|| Bichat. *Recherch. Phys. sur la Vie et la Mort.* Paris, 1800. Part 2, Art. 11, § 1.

maintains the existence of an animal and organic life distinct from each other, and of a nervous system for each of these lives. The system of the ganglions, which he regards in the same point of view with some of the authors above quoted as small brains belonging to the organic life, and the cerebral system to the animal life*. To be consistent with himself, Bishat should have admitted that the heart derives from the ganglions the principle of its motions, but he has not done so. It is chiefly the galvanic experiments which have led him into this inconsistency, because he had attempted in vain to produce contractions in the heart by galvanizing the cardiac nerves. This experiment, however, may always succeed, as was found by M. de Humboldt† in 1797, and three years before, by Mr. Fowler ‡.

The foregoing, the report continues, is a short but faithful account of the principal systems by means of which authors have, since the discovery of the circulation of the blood to this day, attempted to explain the motions of the heart. On taking a general view of those invented before Haller, we remark, that in all of them the nervous power is considered in one way or other as one of the conditions essential to the production of the motions of the heart, and it is always and only in the brain that they place the seat of it. The cardiac nerves therefore had a determined use in all these systems, and one could easily understand why the heart is subject to the empire of the passions; but it was impossible to explain why the circulation continues in acephalous animals, and why in experiments on animals the interruption of all communication between the brain and the heart does not stop the motions of the latter. Since the time of Haller, irritability has been the basis of all these systems. In regarding that property as essential to the

* Bichat, *Recherch. Phys. sur la Vie et la Mort.* Paris, 1800. Part 1. Art. 6. § 4. and Art. 1. § 2.

† M. de Humboldt, *Expériences sur l'Irritation de la Fibre Nerveuse et Musculaire,* publiées en 1797; et *Traduites en Française deux ans après,* tom. 1., chap. 9.

‡ *Experiments on Animal Electricity.* By Richard Fowler, 1794.

fibre, and independent of the nervous influence, the circulation in acephalous animals and the different phænomena observed in the experiments alluded to, present nothing that is not easily understood; but the use of the nerves of the heart, and the influence of the passions on that organ become inexplicable. The necessity of removing these difficulties has produced two parties among the supporters of irritability. The one zealous favourers of the doctrine of pure irritability called to their aid the most improbable hypotheses, and all their efforts have only served to prove how difficult it is to support the cause they espouse. The other confounded the nervous power with irritability, which they consider as one of the functions of that power; but they have been obliged to admit, either with respect to the seat or the mode of existence of the nervous power, conditions, which by their own confession are far from being demonstrated, respecting which they are not agreed, and which in the application they make of them to the motions of the heart, either do not wholly remove the old difficulties, or create new ones."

Such is the very accurate account of the state of our knowledge respecting the nature of the power of the heart, and the relation which subsists between it and the nervous system given by the Committee of the Royal Academy of Sciences, at the time when M. le Gallois discovered that, by crushing the spinal marrow, the power of this organ is so enfeebled that it can no longer propel the blood; from which, compared with the fact that the power of the heart continues after the removal of the brain, he inferred that it is derived from the spinal marrow *. He thus explains the use of the cardiac nerves, and why the heart is affected by the passions, the spinal marrow being under the influence of the brain; why the circulation continues in acephalous animals; and why we do not destroy the power of the heart by removing the brain. M. le Gallois' experiments were repeated with the same results in

* M. le Gallois *sur le Principe de la Vie Notamment sur celui des Mouvemens du Cœur et sur le Siège de ce Principe.*

the presence of the above-mentioned committee. These results, however, do not seem to warrant the inferences which both he and that committee draw from them.

There are two ways in which we may account for the power of the heart being destroyed in M. le Gallois' experiment. Either it derives its power from the spinal marrow, and consequently loses it, on the destruction of the whole or a considerable part of this organ, or deriving its power from some other source, it is influenced by agents affecting the spinal marrow. It was incumbent on M. le Gallois therefore to ascertain by experiment in which of these ways crushing the spinal marrow produces the effect he observed. In order to ascertain whether the spinal marrow possesses a power over the heart not possessed by the brain, the brain and spinal marrow should have been placed under the same circumstances in M. le Gallois' experiments. They ought both to have been removed, or both crushed.

On placing them under the same circumstances, I found the relation they bear to the heart the same*. Its power is nearly destroyed by crushing either, but uninfluenced by the removal of either. When the head of a rabbit is cut off, after a ligature has been thrown round the neck to prevent hemorrhagy, the heart continues to beat with unimpaired vigour. But when the brain is suddenly crushed, the power of the heart is instantly so enfeebled, that it can no longer propel the blood. When, on the other hand, the spinal marrow, instead of being suddenly crushed as in M. le Gallois' experiments, is slowly destroyed or merely removed, the action of the heart continues unimpaired †. It thus appears that the power of the heart is equally independent of the brain and spinal marrow, but may be influenced through either ‡.

Similar observations apply to M. le Gallois' opinion of the

* *Experimental Inquiry into the Laws of the Vital Functions,* second edition, Exper. 19, 20, 21. † *Ib.* Exper. 2, 3, 4, 5, &c.

‡ Both M. le Gallois and the Committee forget what the latter had just stated, that the heart continues to beat after it is removed from the body.

vessels of the different parts of the body deriving their power from the corresponding parts of the spinal marrow. That the vessels possess a power capable of supporting the motion of the blood, independently of the heart, appears from direct experiment *, and this power seems to obey the same laws with that of the heart. Both the brain and spinal marrow may be removed, or slowly destroyed, without impairing it†, yet it is immediately enfeebled or destroyed by suddenly crushing either of these organs ‡. It also appears from experiments related in the Inquiry just referred to, that the excitability of the alimentary canal is equally powerful after as before the removal or slow destruction of the brain and spinal marrow§. The same, we know, is also true of the muscles of voluntary

* *Exper. Inq.* Ex. 24. 62. 63. I wish particularly to impress on the reader, that it appears from the experiments here referred to, that both in warm and cold-blooded animals the capillary vessels are capable of performing their part in the circulation, after the removal of the heart, provided a ligature had been previously thrown round all the vessels attached to it, and seem to perform it as perfectly as in the healthy animal ; for we cannot observe, in such experiments, that the velocity of the blood in these vessels is immediately lessened, or in any other way changed, by the removal of the heart ; a proof that the circulation in the capillaries depends little, if at all, on the power of that organ. If medical writers would keep these results in view, it would prevent many of the observations daily made on the nature of inflammation, on which, as far as I am capable of judging, more fallacious reasoning, and a greater disregard of well-established facts, have been displayed than on any other subject of equal importance in our profession. They began with Mr. Hunter's classing a morbid, with a natural process, and have taken a thousand shapes since his time. But their authors should recollect that, unless they can disprove the positions, ascertained by the simplest, and consequently least equivocal, experiments, that any cause lessening the action of the capillary vessels causes, and any cause exciting these vessels relieves inflammation, what they say can be of no weight. If their arguments cannot otherwise be answered, which I believe would be no difficult task, an appeal to the simple matter of fact is always at hand.

†. *Exp Inq.* Exper. 12, 13. ‡ *Ib.* Exper. 28, 29.

§ *Exper. Inq.* Exper. 46, 47. Mr. Andrew Knight observed to me, that he has frequently seen the peristaltic motion of the intestines of the rabbit strong after they were taken out of the body.

motion, hich are as excitable by artificial stimuli after, as before the removal of these organs.

It would, at first view, appear a legitimate inference from the foregoing facts, that the power of the muscular fibre is every where independent of the nervous system. The opponents of the doctrine of Haller, however, have objected to this inference, because, although in such experiments the muscle is prevented from receiving more nervous influence, it is not deprived of that already bestowed on it, either forming a necessary part of the fibre itself, or dispersed through its substance, in nerves too small to be removed; and this objection appears to be greatly strengthened by the circumstances already mentioned, that after a muscle is separated from the body it soon loses its excitability, and that those muscles whose functions are supported by stimuli peculiar to themselves, are notwithstanding supplied with nerves. It occurred to me, that this question could only be determined by some experiment which should ascertain whether the excitability of muscles is maintained by the influence they receive from the nervous system, or impaired, as it is found to be by other stimuli; for if, on the one hand, it can be proved that the permanency of their excitability is unimpaired by cutting off all supply from the nervous system; and, on the other, that the influence of that system exhausts it as other stimuli do; a doubt, I conceive, cannot remain, respecting the dependence of muscular power on the constitution of the muscular fibre itself. The thirty-second experiment, related in the *Experimental Inquiry* just referred to, appears to answer these questions in the affirmative, and therefore to prove the independent power of that fibre.

An inference from the facts which have been laid before the reader, in which we cannot be deceived, is that, contrary to the opinion of Haller, the heart may be directly influenced through either the brain or spinal marrow; and this position, we shall afterwards find, may be as easily illustrated by the effect of much less powerful agents on these organs, as by the experiments above referred to. The same observation, we shall

also find, applies to the blood-vessels. The motions of the alimentary canal also, we have seen, are uninfluenced by the removal of the brain and spinal marrow; yet we see them influenced by affections of the mind, and cannot doubt that this admits of the same explanation with the influence of these affections on the heart, namely, the direct influence of the nerves on their muscular fibres.

The foregoing facts afford an easy solution of the difficulties stated in the report of the Committee of the Royal Academy of Sciences. The heart continues to act for some time after it is removed from the body, and performs its functions in the fœtal state, when no brain, and, as the committee ought to have added, no spinal marrow has existed, because it has no direct dependence on any part of the nervous system. The heart is supplied with nerves, and subject to the influence of the passions, because, although independent of this system, it is capable of being influenced through it.

Precisely the same laws obtain, with respect to the muscles of voluntary motion. It appears, from the experiment just referred to, that their power is equally independent of the nervous system, and constant experience proves that they are under its immediate influence.

In other respects, however, the laws which the muscles of voluntary, and those of involuntary, motion obey are different. It appears, from what has been said, that the two sets of muscles differ in the nervous influence being the sole stimulus of the former, while the latter, in all their usual functions, are each excited by its peculiar stimulus, that influence only occasionally affecting them. We shall also find them differing in the manner in which the nervous influence is supplied to them. But to say more here would too much anticipate that part of the subject which we are next to consider.

Much has been said of the cause of the one set of muscles being subjected to the will, while the other is independent of it; but if the mind be freed from preconceptions, we can surely be at no loss to account for this difference, when we know that the muscles of involuntary motion are all exposed to the con-

stant, or constantly renewed action of stimuli over which the will has no power; while the sole stimulus of the muscles of voluntary motion is wholly subjected to it. Besides, the action of the former muscles produce no sensible effect. We will to move a limb, not to excite a muscle; we wish to handle, for example, and on trial find that we can move the fingers, but there is no act of volition which could be performed through the medium of the heart and blood-vessels. If we had no wish to handle, the muscles of the fingers, of course, could never have become subject to the will. Few have any command over those of the external ear; and it deserves to be remarked, that the will influences the rectum and bladder, the only internal organs which can assist in accomplishing an end desired. It seems unnecessary to add, after what has been said, that the ganglions by no means intercept the influence of the brain and spinal marrow in its course to the muscles of involuntary motion, as some have supposed.

From all that has been said, it follows that the muscular power in every part of the body, depends on the constitution of the muscle itself, but is also in every instance under the influence of the nervous system. Let us inquire whether, in other parts of nature, any analogous power exists. It is, of course, in animated nature alone that we can look for it. It might not be difficult, perhaps, to prove that a power exists in the vegetable world, in all essential respects analogous to that of the muscular fibre, but this subject does not come within the scope of the present paper. The analogy which the coagulation of the blood bears to the contraction of that fibre, demands more attention here.

I shall only refer to the arguments of Mr. Hunter, for the vitality of the blood, a position so clearly established by this great physiologist *, that we may in our day safely assume it as one requiring no additional proof, but I shall beg leave to quote his observations on the analogy observed between muscular contraction, and the coagulation of the blood.

* Mr. Hunter's *Treatise on Inflammation and Gun-shot Wounds*, vol. I.

"As the coagulation of the blood appears to be that process which may be compared with the action of life in the solids, we shall examine this property a little farther, and see if this power of coagulation can be destroyed. If it can, we shall next inquire if by the same means life is destroyed in the solids, and if the phenomena are nearly the same in both. The prevention of coagulation may be effected by electricity, and often is by lightning. It takes place in some deaths, and is produced in some of the natural operations of the body, all of which I shall now consider.

"Animals, killed by lightning, and also by electricity, have not their muscles contracted. This arises from death being instantaneously produced in the muscles, which therefore cannot be affected by any stimulus, nor consequently by the stimulus of death. In such cases the blood does not coagulate; animals who are run very hard, and killed in such a state, or, what produces still a greater effect, are run to death, have neither their muscles contracted, nor their blood coagulated; and in both respects the effect is in proportion to the cause *.

"I had two deer run till they dropped down and died; in neither did I find the muscles contracted, nor the blood coagulated.

"In many kinds of death we find that the muscles neither contract, nor the blood coagulates. In some cases the muscles will contract, while the blood continues fluid; in some the contrary happens, and in others the blood will only coagulate to the consistence of cream.

"Blows on the stomach kill immediately, and the muscles do not contract, nor does the blood coagulate. Such deaths as prevent the contraction of the muscles, or the coagulation of the blood, are, I believe, always sudden. Death from sudden gusts of passion is of this kind, and in all these cases the body soon putrefies after death. In many diseases, if accurately attended to, we find this correspondence between muscles and blood *."

* Mr. Hunter's *Treatise on Inflammation and Gun-shot Wounds*, vol. I.

To these analogies, Mr. Hunter might have added, that, as the living muscular fibre is capable of repeated contraction and relaxation, the blood, while it retains the vital principle, is capable of repeated coagulation and liquefaction. It is found in part coagulated in animals which have lain long in a torpid state. " Mr. Cornish," Mr. Hunter observes, " found that in bats, in the torpid state, the blood was, in a certain degree, coagulated, but soon recovered its fluidity on motion and heat." In the effects of galvanism we see other striking analogies between the contraction of the muscular fibre and the coagulation of the blood. It produces both more readily than any other artificial means, and when applied in excess equally destroys the powers on which both depend. We may add that the labours of the chemist have shewn that it is chiefly from that part of the blood in which the coagulating power in question exists that the muscular fibre is formed.

The close analogy between the coagulation of the blood and that of other coagulable fluids cannot be overlooked; so that it seems a necessary inference that the contraction of the muscular fibre is the effect of a power similar to that by which certain fluids coagulate. But both the contraction of the muscular fibre and what is called the spontaneous coagulation of the blood belong to those results which are peculiar to matter endowed with the vital principle. Although both may be produced by chemical agents, it is only while the vital principle survives that any cause can produce them.

The effects of inanimate agents acting on living matter, I cannot help thinking, will, if duly investigated, throw considerable light on some parts of the animal economy. It will, as far as I am capable of judging, appear in the prosecution of this subject that the vital functions of animals may be divided into those in which an inanimate agent affects a vital part, and those in which vital parts affect each other. The results in both cases are of course equally vital actions. It is equally impossible to obtain them after the extinction of the vital principle; but in the former, as might, *à priori*, be expected, we

find something analogous to the effects of the same inanimate agent acting on inanimate matter, in the latter nothing analogous to any of the effects of such an agent.

[To be continued.]

Art. X. *On the Ventilation of Rooms, and on the Ascent of heated Gases through Flues. By* Davies Gilbert, Esq., F.R.S.

[In a Letter to the Editor.]

Dear Sir,—The following investigations were made several years ago, on the occasion of ventilating both houses of parliament, with the hope of discovering some principle, that might guide architects in their attempts to change the air of crowded apartments; an object of considerable importance to the health as well as to the comfort of all persons frequenting such places. The same formulæ apply to the ascent of gas from furnaces: the whole is, however, such a mere trifle in respect to science, that I should not have entertained a thought of troubling you with it, were I not persuaded that the conclusions will be found of some practical use.

In this, as in all such attempts, the most simple cases must be selected for calculation, leaving their various complications, modification, and local circumstances to the judgment and direction of each individual operator.

Let atmospheric air, or any gas be supposed to ascend through smooth, even, and regular tubes, with an equable temperature, and consequently with the same rarefaction throughout.

Let $x =$ the rarefaction.

$g =$ the specific gravity of the elastic fluid, as compared with atmospheric air.

then $\frac{g}{x}$ the density.

$h =$ the height of an uniform atmosphere 26058 feet, log. 4.4159412.

$m =$ the velocity with which atmospheric air rushes into a vacuum, or 1295 feet in a second, log. 3.1122698.

$l =$ the perpendicular height giving velocity.
$v =$ the velocity in feet in a second; then
$\frac{l}{h} \times \left(1 - \frac{g}{x}\right)$ the tendency to ascend, provided that x is greater than g.

$v = \sqrt{\frac{l}{h}} \times m \sqrt{1 - \frac{g}{x}} \times \sqrt{\frac{x}{g}} = \sqrt{\frac{l}{h}} \times m$ $\sqrt{\frac{x}{g} - 1}$. And the quantity of matter actually issuing through an aperture of one foot square in a second, reduced to atmospheric density, and $= \sqrt{\frac{l}{h}} \times m \sqrt{\frac{x}{g} - 1} \times \frac{g}{x}$ $= g \sqrt{\frac{l}{h}} \times \frac{m}{x} \times \sqrt{\frac{x}{g} - 1}$ cubic feet. When this expression becomes a maximum, it is obvious that the variable part $\overline{\frac{x}{g} - 1}^{\frac{1}{2}} \times x^{-1}$ must be so too, and consequently $\overline{\frac{x}{g} - 1}$ $\times\ x^{-2}$, put then, $x^{-2} \times \frac{\dot{x}}{g} - 2 \times \overline{\frac{x}{g} - 1} \times x^{-3} \dot{x} = 0$ where $\frac{1}{g} = 2. \overline{\frac{x}{g} - 1} \times x^{-1}$. And $x = 2g$. At the point, therefore of expansion producing the greatest change, The velocity for air and all gases will be the same $= \sqrt{\frac{l}{p}} \times m$. And the quantity of matter issuing will be the same also $= \sqrt{\frac{l}{p}} \times \frac{m}{2}$.

In the case of atmospheric air, $g = 1$, and $x = 2$. Now, if the expansion of elastic fluids is $\frac{1}{480}$th for each figure of Fahrenheit's thermometer, $\overline{\frac{481}{480}}\Big|^{n} = 2$. And $n \times \log. \frac{481}{480} =$ $.30103 \because n = \frac{.30103}{\log. 281 - \log. 480} = 333°$ of temperature above the surrounding atmosphere. In other gases the equation will be $\overline{\frac{481}{480}}\Big|^{n} = 2g$. And $n = \frac{\log. 2\,g}{\log. 481 - \log. 480}$. The log. 481 — log. 480 $= 0.0009038$, the reciprocal of this logarithm 1106,4,

the log. 3.0439084. If the elastic fluid has a density one-half of the surrounding medium, or $g = \frac{1}{2}$, the greatest escape will be when the temperatures are equal: and when the specific gravity is less than $\frac{1}{2}$ the degrees of temperature will become negative.

Thus hydrogen, with a specific gravity $0.0694 = g$, gives $x = 0.1388$ $n = -949°$, and the greatest quantity will escape by its levity through a given aperture at the temperature of 949° below the surrounding atmosphere; on the supposition that temperatures and densities continue through so extensive a range to be connected by the same law.

In applying these theorems to the case of ventilating an apartment, little attention need generally be bestowed on minute accuracy. The tendency to ascend, which must be the same in a room as through a tube of equal length, may be deduced from the mean of three or four observations on the temperature made at different elevations; and the height of the room should include the perpendicular length of any apparatus for conducting the air into the atmosphere; and, if it is thought of sufficient importance, a value may be assigned to g, corresponding with the subtraction of oxygen, and the presence of carbonic acid, then substituting the numerical values for $\frac{l}{h} \times \overline{1 - \frac{g}{x}}$. The square root of this quantity multiplied by m, and divided by the square root of the density of the upper stratum of air, will give the velocity; and this again multiplied by the density of the same stratum, and by the section of the aperture in square feet, will give the number of cubic feet issuing in a second, reduced to atmospheric density. In practice, however, it will be quite sufficient to use a mean density throughout.

To assume a case, let it be required to extricate from an apartment sixty feet long, thirty feet wide, and thirty feet high, a quantity of air equal to its whole content of atmospheric density in an hour, by means of an average increased temperature of 20° of Fahrenheit. The rarefaction for 20°, or

$x = \overline{\frac{481}{480}}\Big)^{20} = 1.0425$. Then $\sqrt{\frac{l}{h}} \times m \times \sqrt{x-1}$, or $\frac{m}{\sqrt{h}}$ $\times \sqrt{l} \times \sqrt{x-1} =$ the velocity. But $\frac{m}{\sqrt{h}} = 8.0223$ (log. 0.9042992) $\sqrt{l} = \sqrt{30}$ and $\sqrt{x-1} = \sqrt{0.0425}$, therefore $v =$ 9.058 feet in a second, and these multiplied by $\frac{1}{x}$ or $\frac{1}{1.0425}$ will give the quantity of elastic fluid reduced to atmospheric density passing through an aperture of one square foot in a second $=$ 8.680 cubit feet, or $=$ 31280 cubic feet in an hour: but the content of the room is $30 \times 30 \times 60 = 54000$, requiring therefore an aperture of 1.726 square feet, being a square of 1.31, or a circle of 1.48 diameter.

This communication should not lead by a straight perpendicular ascent into the open air; as in that case currents of cold air will inevitably pass into the room, in consequence of their superior gravity. It seems probable that the best expedient is to conduct the tube, furnished with a moveable cover capable of elevation and depression, into an upper room, from whence external openings may be made on its different sides, to be used according to the direction of the wind.

Now, it is quite obvious that while 54.000 cubic feet of air have been flowing into the atmosphere, an equal quantity must have been admitted to supply their place; this supply can very seldom be provided in any other way than from without, which rushing into the apartment with a velocity nearly equal to that already found, produces great inconvenience in crowded rooms, and endangers the health of more delicate persons. One obvious expedient is to subdivide the current into numerous streams; and if these were made to enter the sides of the apartment at perhaps half its height, and then directed upwards, they would descend from the top with an increase of temperature, and with considerable uniformity. The section of the aperture must however in that case be enlarged by the square root of two above those in the ceiling, which latter should themselves receive some increase of size, to

compensate for the loss of velocity occasioned by the heat subtracted from the ascending air.

The ascent of gases through flues of furnaces may easily be ascertained in the same general manner, attending only to the specific gravities of the compound fluids.

Suppose the whole combustible matter to be pure carbon, and that it converts the entire oxygen of the atmospheric air passing through the furnace into carbonic acid.

Then, since 5.7 parts by weight of carbon unite with 15 of oxygen, (the sp. gravity of which is 1.1175), into the same bulk, the specific gravity of carbonic acid must be 1.542 : but the nitrogen, which constitutes 77 parts in an hundred of atmospheric air remaining unaltered with a specific gravity 0.9649, the specific gravity of the whole will be 1.0874. And 1 part by weight of carbon will require for its complete saturation 2.63 parts of oxygen, or 11.44 parts by weight of atmospheric air. Let the instance taken be a smelting furnace, heated to 1500° of Fahr. above the surrounding atmosphere, which is probably about the temperature of a copper furnace, and let the consumption be twice 84 pounds of carbon in an hour. The rarefaction will be $x = \overline{\frac{481}{480}}\Big|^{1500} = 22.68$ $g = 1.0874$ $\frac{x}{g} = 20.86.$ Let l or the length of the stock be 40 feet, then $v = 226$ feet in a second, and the quantity of matter, reduced to atmospheric density, issuing through one square foot in a second will be 10.84, or in an hour 39022 cubic feet. But, from what is stated above, 168 pounds of carbon will unite with the oxygen of 1922 pounds of atmospheric air, and assuming 13.2 cubic feet, as equal to a pound at a mean temperature, the whole will constitute 25.375 cubic feet; but these must be increased in the proportion of the specific gravity which makes them 27.591. And the quantity issuing in an hour has been found to be 39022, therefore the aperture must be of about 102 square inches.

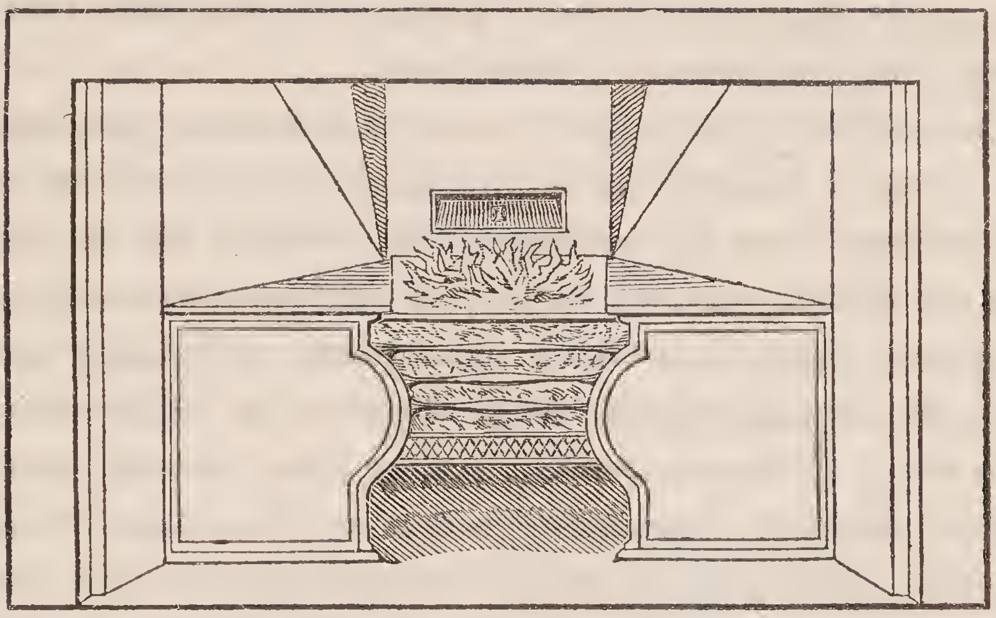
At the degree of temperature producing the maximum effect, the same weight of these electric fluids would pass through an aperture of rather less than half the size : but, if the matter

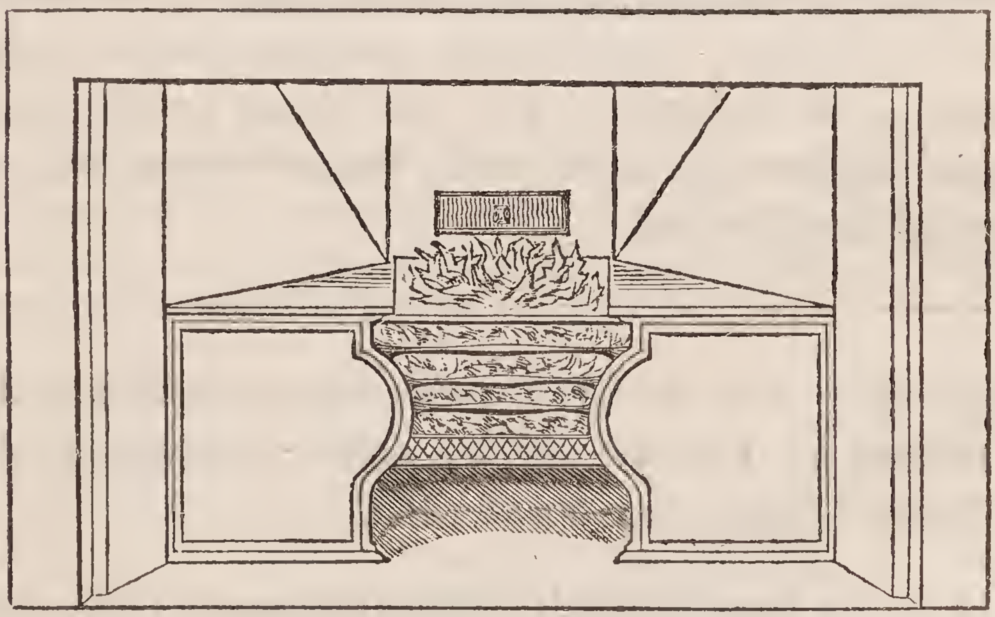
existed in the form of hydrogen, at the temperature 1500° above the surrounding medium, it would require an opening of two square feet and three-quarters, almost quadruple to the former case.

The narrowest part of the flues in some furnaces consuming coal at the full rate of two bushels, or 168 pounds in an hour, is about 200 square inches, just double to the aperture found by theory. It would, therefore, in all probability be sufficiently large, if the fuel were pure carbon or even coke, but is scarcely so perhaps when fresh coal abounding in hydrogen, and generally not free from water, must be continually added to the fire. It seems probable that the heat of furnaces would be more uniformly sustained if the flues were made larger and regulated by dampers, especially as no allowance has been made in the investigation, for those retarding powers encountered by all fluids in passing through tubes, and in turning angles. It may further be remarked, that in all flues the aperture should be largest where the temperatures has attained the greatest range. Having given the consumption of coal and the temperature, these formulæ extend to all closed furnaces ; but it does not seem possible to apply general theorems to the case of heated gases ascending from open fire-places, because of the uncertain quantity of common air necessarily blended with them : the following observations, however, appear naturally to present themselves.

When currents of air find their way down a flue, in conseqeunce of reverberation from adjacent buildings, or in consequence of a direction inclined to the horizon, which may have been acquired by passing over more lofty obstacles, remedies must be sought by elevating the stack, or by the use of mechanical contrivances. When ample space is required round a fire for culinary purposes, the flue must continue all the way of a proportionate size, and so terminate, or the whole of the elastic fluids will not be able to ascend, and to escape into the air. But, when the only purpose of a fire is to diffuse warmth through an apartment, the line of communication into the chimney should be reduced to much narrower limits,

which, to produce the greatest effect, should vary with the state of the fire: whether it is just kindled, has fresh coal thrown on it, or whether it burns clear and with a red heat; and these cases have recently been met by the contrivance of a moveable plate inclined forwards, so as to reach within about three inches of the front of the chimney, but capable of being pushed back against the further wall. To afford the means of a still more accurate adjustment; a small moveable plate is frequently inserted towards the lower part of the large one.





It seems clear that the section of the flue should at the least exceed these as four-fold, the average size of the aperture communicating with the fire-place, but that at the top it should be much contracted. All descent of cold air into the chimney will then be prevented, which, by reducing the temperature,

like extraneous air ascending, would equally retard the draft; and the effects of irregular, temporary movements of the atmosphere will be rendered insensible. These appear to be the chief advantages derived from earthen tubes, very frequently seen on the tops of chimneys, which in most cases make but a very insignificant addition to the length of the flue.

Those stoves or fire-places are constructed on the best principles which throw the greatest quantity of radiant heat into the rooms they are intended to warm, and at the same time take away the least quantity of heated air.

In concluding this paper it must be remarked, that although the formulæ deduced from the height of an uniform atmosphere, and from the velocity of air rushing into a vacuum, are not perfectly correct, under the actual circumstances; yet they have been ascertained to approach sufficiently near to the truth by experiments on the supply of air afforded to blast furnaces. Differences, much greater than any to be apprehended in these expressions, will arise from the state of the weather. Thus, in a calm it is probable that the rarefied column of air, ascending from a stack, must add somewhat to its virtual height; while, on the contrary, a rapid horizontal motion of the atmosphere will each instant present fresh obstacles against the ascent of the heated vapour, and render the draft less powerful.

ART. XI. *On the Effects produced on the Human Countenance, by Paralysis of the different systems of Facial Nerves.* By JOHN SHAW, Esq.

IN the last Number of this Journal, an account of the anatomy of the nerves of the face, in several animals, was given, and some experiments were detailed, to shew that very different results ensued when the nerve, called respiratory of the face was cut, from those which followed the division of the 5th pair.

A few cases will now be related, to prove that the changes

produced on the human countenance in palsy depend upon the set of nerves affected, and that the two systems of nerves are seldom or never paralyzed at the same time.

It will also, by the description of the same cases, be proved that if those actions which are regulated by the set of nerves unaffected, be excited, the distortion of the countenance, caused by the paralysis of the other, will disappear during the excitement.

The first example will be of paralysis of those actions of the muscles which are governed by the portio dura, or respiratory nerve of the face. The next will be of a case of common palsy, or hemiplegia, after apoplexy, in which the branches of the 5th pair are the only nerves of the face affected.

To these will be added the description of a very uncommon case, where, in consequence of injury to both sets of nerves, there was double or total paralysis of one side of the face. A slight sketch of several cases will then be offered, to prove what was also advanced in my last communication, "that by a knowledge of these facts, it may be expected, we shall not only be able to form a more correct diagnosis of the nature and seat of palsy than heretofore, but also to estimate the degree of danger attending each class of symptoms.

Case of complete Paralysis of those Actions of the Muscles of the Face, which are regulated by one of the Nerves of the superadded or additional system, viz., *the Portio Dura, or Respiratory Nerve of the Face.*

Rebecca Larkin, aged 12, daughter of a marble-cutter in Cirencester-place.—This is a pretty little girl, in full health, and, according to her mother's account, is a good scholar, and an adept at her needle. When she is not speaking or smiling there is nothing remarkable in her countenance; but when she laughs, her face is much distorted, in consequence of the muscles of the left side only, being in action. The cause of the paralysis in the muscles of the right side is rather obscure. The mother's account is, that when the girl was about two years' old, she was taken suddenly ill, and lost the use of her limbs, but that

in about three or four months she regained the power over them, and has since that time been in very good health. She cannot recollect how long it is, since the child's face became distorted, she noticed it for the first time about seven years ago.

When the girl laughs heartily, the right cheek and right side of the mouth are unmoved, while the muscles of the left side are convulsed with laughter. If she be told to try and laugh with the right side, she raises the angle of the mouth, but by an action which is evidently regulated by the 5th pair. This attempt to laugh gives a peculiarly droll expression to her face, and, as I have said in a former communication, is probably the same action as that with which we are so much amused, in observing the face of a famous mimic. When the girl attempted to whistle, she could not close the right side of her mouth; still she could purse it up (this action of the orbicularis muscle being regulated by the 5th,) so as to hold a whistle which I put between her lips, and then by blowing, she could sound the whistle, but in doing this, there was no action perceptible in the right cheek, for it was distended like part of a leathern bag, while the muscles of the left were in full action.

In this experiment it was shewn, that the muscles of the right side, were in a state similar to those of the cheek of an animal in which the portio dura of the same side has been divided, since their actions were paralyzed as far as they depended on this nerve.

My next inquiry was, how far those actions of the same muscles which are regulated by the 5th nerve were perfect.

The action of the right buccinator, during the time the child was eating, was not only distinct, but she even preferred chewing her food upon the right side. As this was hardly to be expected, I inquired if there was any thing the matter with her teeth, which could induce her to eat, only on the right side. Her mother told me that she had frequently gumboils on the left side; but the little girl, with much acuteness, perceiving the object of my inquiry, said she had always chewed her food on the right side, and she recollected that her grandfather used to scold her for doing so, long before she had gumboils.

The circumstances already detailed are sufficient to prove that the same muscles of the mouth, that were paralytic in those actions which are subservient to expression and respiration, were perfect in any voluntary action. The degree of sensibility in the two cheeks was exactly the same, which was the only additional circumstance necessary to shew that the 5th nerve was perfect in all respects, on the lower part of the right side of the face.

I next examined the state of the nose. When ammonia was held to the nostrils she inhaled it only with the left; the irritation caused by the fumes was followed by all the symptoms of sneezing, but the expression was confined to the left side of the face. The left nostril was then closed, and the ammonia held to the right, but she could not snuff it up, nor was there the slightest symptom of sneezing produced, though the fumes of the ammonia were so strong as to make her eyes water.

This was so similar to the experiment on the ass, that I thought it a conclusive proof of the respiratory actions of the muscles of the right nostril being destroyed. To try the power of the branches of the 5th over this division of the nose, I tickled the inside of the same nostril with a feather, and then all the symptoms of being about to sneeze, were produced on the opposite side of the face.

On examining the state of the eyelids, I found that the orbicularis oculi of the right side was much weaker than the left; it appeared wasted, and this probably from want of use, as she cannot exert it as she does the other, nor is there any action observed in it, when she sneezes or laughs.

I should, however, state, that though she cannot frown nor knit the brows on this side, in unison with those of the other; still she has a certain power over the muscles of her forehead, but apparently analogous to the same action by which she attempts to laugh with the right cheek, nor can she close the eyelid when she is startled, all of which I may presume are in consequence of the diseased state of the portio dura, since the symptoms in

every respect correspond with those in the dog, whose portio dura was cut. She is also, like the monkey, in which the nerve was cut, obliged to wipe the right eye very often.

I may here observe, that this girl has entirely lost the sight of the right eye, although the iris is as perfect in its motion as that of the other eye. It is well known to those who are conversant with the history of the disease called amaurosis, that the common idea of the pupil being always fixed in amaurosis is erroneous. No explanation has yet been given, why the pupil should, in some cases of this disease, remain fixed and insensible to the different degrees of light, while in others it continues as sensible to the stimulus of light as that of a perfect eye. The question is very obscure, but I think that certain cases of paralysis of the different nerves of the head, go to prove that there is much less sympathy between the retina and the iris, than is generally supposed to exist. Perhaps we shall be able ere long, to offer some facts in explanation of this phenomenon.

After having ascertained that those actions which are regulated by the respiratory nerve, (which is one of the superadded class,) were the only ones paralyzed on the face, I examined the state of the muscles of the limbs and body, (which are supplied with nerves from the original class,) and I found them to be in every respect perfect.

I have detailed the symptoms in this case, because it was one of the first observed, after entering upon the inquiry. But as there is amaurosis of one eye, and as, at the commencement of the disease, the limbs were also affected, it is not so fair a case of simple paralysis of the respiratory nerve of the face as others, which I shall presently adduce in support of the opinion, that this particular affection is generally independent of any disease in the brain.

I shall now describe the state of the muscles in a case of hemiplegia, following an attack of apoplexy.

The symptoms observable in a case of common palsy, are so familiar to every one, that it might be thought superfluous to

enumerate them; but it is necessary, in this inquiry, that we may compare them, with the phenomena observable in cases of partial paralysis.

In the following case, (which nearly corresponds with several others I have examined,) the symptoms will be described in the order that has been followed in the last.

J. Cooper.—This man's general appearance is completely that of an old paralytic, but the distortion of his face is more remarkable than usual, in consequence of the right or paralyzed side being marked with a red blotch.

The arm and leg of the same side are nearly powerless, his intellects are much impaired, and his memory gone. The history of his case was given very clearly by his wife; according to her account, her husband was, for the first time, attacked with apoplexy about seven years ago; from this attack he gradually recovered, but at the end of twelve months he was a second time seized, and, since that period, he has had two distinct attacks every year; for the last two or three years, he has been nearly in the same condition as at present.

State of the cheeks and mouth.—When he is made to laugh, the right cheek rises in the same degree with the left; when he blows, (he always burst into a laugh when asked to whistle,) the buccinator of the right cheek is in as much action as on the other side. When his nose is irritated by the inhalation of ammonia, the actions of the muscles, preparatory to sneezing, are equal on both sides of the face. These phenomena prove that the muscles of both cheeks are perfect in their actions as far as they are regulated by the respiratory nerve; they may be compared with the state of the same muscles in the case last related, when the act of sneezing was excited.

The next inquiry was, how far those actions were entire, which are dependant on the branches of the 5th pair.

The right cheek, and the same side of the mouth, fall lower than the left. When a piece of bread was put between the teeth and right cheek, the patient could not push it from its place with the buccinator muscle, but was obliged to pick it out with

his tongue; but when the bread was put into the left side, he could easily dislodge it by the action of his cheek. The saliva constantly flows from the right side of his mouth, and when drinking, part of the fluid escapes from the same side. The paralysis of the orbicularis oris was farther shewn by the inability to hold my pencil, or a tobacco-pipe, in the right side of his mouth.

I next examined the comparative degree of sensibility in the two cheeks; when he was pricked on the right cheek with a needle, he seemed perfectly callous, even though I drew blood, but on giving the least prick to the left side, he immediately started; the same difference in the degree of sensibility was observable in pulling a hair from each whisker, (the sensibility of the right and left limb corresponded with that of the cheeks.)

Having now satisfied myself, that the power of the 5th pair was not only deficient, as far as it controls the actions of the muscles of the mouth and cheeks, but also in giving sensibility to the skin, I proceeded to examine the state of the

Nose.—On putting hartshorn to the right nostril, he inhaled it as well as with the left, and immediately all the symptoms observable in a person about to sneeze were presented. As the nose was turned up, and the alæ nasi of both sides were equally in action, this was a sufficient proof of the state of the paralyzed side being very different from that of the last case related. The power of the 5th over the nose was tried; by tickling the inside of the right nostril, no effect was produced; but on tickling the left nostril, the symptoms of sneezing were again evident. It is almost needless to beg the reader to compare the phenomena, in this case with those observable in the little girl.

The eye and eyelids.—The ball of the eye followed the pencil as it was carried before him. I forgot to examine the state of the pupil, but in other cases of hemiplegia, I have found the pupil of the paralytic side to be more dilated than the other; however, I wish to avoid the discussion of this question at present, as we have not yet been able to unravel the intricacy of the several nerves, which supply the eye-ball and its

muscles. I shall therefore confine myself to the examination of the state of the eye-lids.

He could close the eye-lid of the paralyzed side as well as the other; and when his nose was irritated by the hartshorn, or when he laughed, the orbicularis oculi, and corrugator supercilii, were in complete action, so that the heaviness in the expression of the upper part of the face, which is so remarkable in paralytic persons, disappeared. Here then was proof that those actions of the eyebrows which we find to be deficient, when the portio dura is affected, are, in a case of common palsy, quite perfect; indeed, we may have daily opportunities, while walking in the streets, of observing that patients with palsy of one side of the body, have no difficulty in closing the eyelids.

The levator palpebræ, which is partly supplied by the 5th, is, to a certain degree, paralyzed; but the very common occurrence of paralysis in the levator palpebræ and iris, without any other symptom of palsy, shews that these parts are not regulated in the same manner as the muscles of the cheeks; the cause of this difference will, perhaps, be explained, when we have made further progress in the inquiry.

State of the tongue.—He appears to have complete power over the muscles of the tongue, as he can push it out or turn it in any direction; and when a piece of bread was put into any part of his mouth, he could pick it out with the tip of his tongue, so that there does not appear to be any defect in the motor linguæ or 9th nerve *.

I next examined the degree of perfection in those powers of

* The 9th nerve has been generally supposed to be for regulating the motions of the tongue; this opinion is, probably, in a great measure correct, but I suspect that the motions of the tongue are not altogether controlled by this nerve. I cut both the 9th nerves in a dog; after this operation, he appeared to lap the milk that was offered to him, but he could not swallow it; he filled his mouth with pieces of meat, but he could not throw a bit over his throat; still he seemed to have a considerable degree of power over the motions of the tongue, and he barked very well. As he had so entirely lost the power of taking food, it was necessary to kill him, to prevent his suffering from the most horrid tantalism.

the tongue, which are regulated by the third division of the 5th pair, namely, taste and sensibility.

The want of common sensibility on the paralyzed side of the tongue, was as evident as in the cheek of the same side.

When a piece of sugar was put on both sides of the tongue, the taste was perceptible only on the left side.

On examination of the state of the shoulders, and of the muscles of respiration on the chest and abdomen, I found that when he was about to sneeze, or when he made a long inspiration, the shoulders of both sides were raised equally ; but when I desired him to lift his right shoulder, he had not more power over it than he had over the leg of the same side, *i. e.*, he could raise it with a considerable effort, but it was evidently the action of a paralytic.

On putting my hands on his ribs when he breathed hard, I could discover no difference in the two sides, but both shoulders and ribs acted in unison with each other.

It will perhaps be granted, that in this experiment there is sufficient to shew, that in the common case of hemiplegia, the respiratory nerves passing to the muscles of the chest are as little impaired as those on the face. The ease with which a patient in this condition breathes, is so unlike to the manner in which an animal respires when the par vagum, or any of the important nerves of respiration, has been injured, that we may be permitted to suppose, that in those cases of apoplexy, from which a patient revives with the loss of power over the limbs of one side, (being in fact, the case of common hemiplegia,) neither the par vagum nor the other respiratory nerves are affected.

Cases illustrative of the latter opinion, will be offered in another communication; in the mean time, I trust, that by the detail of the symptoms in these two examples, (which will be found to be cases of common occurrence,) I have made out my proposition, that *when either system of nerves is attacked by disease. the symptoms are different.* It only requires that a number of cases should be detailed, to prove that *the two systems are seldom or never affected at the same time.*

I shall now describe a case which I suspect is a very uncommon one, but it will more clearly shew the distinction between the two cases already related.

The history of the first part of the case was very carefully taken by my friend Mr. Cæsar Hawkins; it will not however be necessary in the present inquiry to detail the whole of the symptoms. The leading circumstances were the following:—

Phipps, a bricklayer, now living in Brunswick Mews, Bryanstone-square, on the 1st of September, fell from a scaffold thirty feet high. His right clavicle was broken, his right loin and hip were much bruised, and he received a severe contusion on the head, the marks of which were particularly observable in a puffiness behind the right ear, and in bleeding from the same ear and from the nose.

He was in a state of stupor when brought into the hospital, but from this, he recovered in the course of the day. For the two or three first days, he appeared to suffer, only from the effects of *concussion*, never having any of those symptoms which are generally attributed to *compression*. On the fourth day it was observed, that the angle of the mouth was drawn rather to one side, and there was also a degree of inequality in the contraction of the pupils.

On the sixth day it was remarked, that while he was asleep, the right eye was more than half open, while the left was closed.

The notes of the case are very full, up to the 24th of Sept., and shew that the patient had, during the interval, gone through the common series of symptoms which accompany that slight inflammation of the brain, which is often the consequence of concussion.

On the first of October, he was made an out-patient, his face being, at this time, very much distorted. On the 17th of October, I visited him at his own house, and with the assistance of Mr. Hawkins, examined particularly into all his symptoms.

The general appearance of his face was that of a man who

has suffered paralysis from apoplexy. But when he spoke or laughed, the distortion was much increased, the mouth being pulled more to the left side, than I ever saw in any other patient.

The following are the notes that were taken at this time: There appears to be total paralysis of the muscles of the right side of the face. When he smiles or laughs, they are passive, while those of the left are regularly in action. If he attempts to whistle, he cannot close his lips sufficiently; when he blows, the right cheek is dilated, but passive like a distended bladder; he can smoke, by putting the pipe into the left side of his mouth; he throws the smoke out of the right side, but in doing this, the action is all, evidently, in the muscles of the left cheek. These several circumstances were sufficient to prove that those actions of the muscles of the cheek and mouth, which are regulated by the respiratory nerve, were deficient. The next question was, how far the actions of the same muscles, which we conceive to be regulated by the 5th pair, were perfect.

The cheek and mouth hang down, as in the common case of hemiplegia—he cannot by a voluntary act move his cheeks; when a piece of bread is put between the cheek and teeth of the right side, he cannot push it out with the buccinator, but picks it out with his tongue. He cannot hold his pipe or my pencil with the right side of his lips.

Our next inquiry was into the degree of sensibility in the paralyzed side. The difference of the sensibility in the two cheeks was most distinct. When I pulled a hair of the right whisker, he was not conscious of my doing it, but he started immediately on my pulling one from the left. When I pricked his cheeks with a needle, his expression was—"I feel you push against the right side, but in the left you prick me." When he brought his jaws forcibly together, he said he was not conscious of striking his teeth on the right side, though he felt them most distinctly on the left. On examining the state of the nose, we found that it was impossible to excite the muscles of the right nostril to any action. We forgot to try the degree of sen-

sibility in the inside of the nostril, so that we are unable to say whether the branches of the 5th, which pass to the inside of the nose, were paralyzed.

The state of the right eye-lids and eye-brow corresponded with those of patients who have paralysis of the portio dura, for both the orbicularis oculi and corrugator supercilii were so completely paralytic, that he could neither close his eye, nor knit his brow on the right side.

On examining how far the branch of the 5th, which passes to the eye and eye-lids, was affected, we found that the symptoms did not exactly correspond with those observed in the parts regulated by the other divisions of the 5th pair, and particularly in the degree of sensibility; for when a hair was pulled from each temple, or from the eye-brows, the pain felt in the two sides was nearly the same. This and the state of the levator palpebræ led us to suspect that the first division of the 5th was not so much affected as the 2d and 3d divisions of the same nerve. I may farther observe, that neither the temporalis, nor masseter muscles of this side were paralyzed. The reason of this will perhaps be ascertained, by a more minute dissection of the 5th than we have as yet made. The motions of the eye-ball were so far perfect, that he could follow the pencil when it was carried before him, but he could not direct both eyes truly, he saw double. The contraction and dilatation of the pupil of the right eye were much the same as in the other eye.

All traces of the injury behind the ear have disappeared; he was, for some time after the accident, very deaf, but he is quickly recovering from his deafness. This latter part I have copied from my notes made on the 17th of October; but on the 16th of November he was still very deaf on this side. He could not hear the ticking of a watch when it was held close to his ear.

State of his Tongue.—He can put it out and move it in every direction, with the greatest ease: the motions are all apparently correct and natural; he can throw a morsel from one side of the mouth to the other, and towards the throat, and he can pick it out from between his cheek and teeth.

These observations led us to conclude, that not only the motor linguæ, or 9th nerve, but also the glosso pharyngæal were

perfect. We next examined the state of these parts of the tongue, which we suppose are supplied by the third division of the 5th nerve. After closing his eyes, we put a little sugar on each side; we asked him to put his finger to the part of the tongue upon which the sugar was; he put it up to the left side; he was not conscious that there was any on the right. While the eyes were closed, we pricked each side of the tongue with a needle, his expression with regard to the difference of feeling, was the same as that used in describing the state of his cheek.

There does not appear to be any paralysis of the parts about the fauces, since he swallows easily; the state of his larynx is also good, as there is no difficulty in speaking, or any impediment to utterance, farther than what may be explained by the paralysis of one side of his mouth.

Neck and Shoulders.—There are no symptoms of paralysis in any of the muscles of his neck or shoulders; but the observations on the actions of the muscles of the shoulder were at this time imperfect, as his broken clavicle was not yet united; however, on the 16th of November I found the arm so well, that there was no difficulty in making the experiment; and then, although the face was as much paralyzed as ever, I was satisfied that there was not the slightest symptom of palsy, in the actions of the muscles of the shoulder, or of the leg.

This case differs from the common examples of partial paralysis of the face, and also from those of hemiplegia. From the first, not only in there being evident marks of paralysis while the muscles of the face are at rest; but in the power of the muscles being lost during the action of eating, and also in the sensibility of the skin of the same side, being in a great measure destroyed.

The first difference which we observe in it, from one of common hemiplegia, is that the paralysis is confined to the face. Secondly, that the paralysis is on the same side with that on which the head is injured. Thirdly, that the palsy is more evident, when the patient is made to sneeze or laugh. Shewing that in this instance the muscles are deficient, not only in the powers which they receive from the 5th, but also in the

influence given by the 7th. The cause of the paralysis in this case is, I think, to be ascribed not to any affection of the brain generally, but to an injury of the portio dura, and to a great part of the fifth nerve; either at their origin, or in their passage through the bones of the head.

My reasons for forming this opinion are, that the state of the actions regulated by the portio dura, is similar to what is found in cases and experiments where this nerve has been injured or cut*. The bleeding from the ear, and the consequent deafness may be taken as proofs that the petrous portion of the temporal bone through which the nerve passes, has been injured.

The evidences that we have of the 5th having been injured in its transit through the bones are, first, that the paralysis is on the same side of the head, as that struck in the fall; secondly, only those two divisions of the 5th are affected, which are in their course through the bones of the head, nearest to the portio dura. Thirdly, that the hemorrhage from the *ear* and *nose* at the time of the accident, lead us to suspect that there was injury to the deep bones of the face. Fourthly, that the sensations both of common feeling and of taste are destroyed on one side of the tongue, which is not *always* the consequence of apoplexy, but which must happen if the trunk of the third division of the 5th be injured. And lastly, that the phenomena accord with those which are observed when the branches of the 5th are cut.

The symptoms of general affection of the brain appeared to have been caused by that inflammatory state †, which is often

* See the several examples given in my last communication.

† The symptoms observable during the first three months, led me to suppose that the bones through which the 5th and 7th nerves passed had been injured. I have so late as the 1st of February seen this patient, and he had so far recovered the power over the parts regulated by the 5th nerve, that it then appeared possible that the sheaths of the nerves might have partaken of the inflammatory state of the membranes of the brain, or have suffered from extravasation of blood into them. By either of these causes, partial paralysis might have been produced; but still, on consideration of all the circumstances, I am more disposed to adhere to the opinion I first formed.

the consequence of concussion, but which generally subsides without leaving any marks of partial paralysis.

The cases already given will perhaps be considered sufficient evidence in support of the opinions advanced, as far as they relate to affections of the face. I could offer many circumstances to prove, that the same order holds in the paralytic affections of the other parts of the body; but, before the observations could be made in the same methodical manner as those upon the face have been, it would be necessary to give a minute description of the nervous system of each part. I shall, therefore, at present, merely beg my reader to observe, the degree of perfection, with which the organs of respiration and circulation (which are supplied with nerves from the superadded system), perform their functions in a patient, who has entirely lost the use of the limbs of one side.

Several cases might be added to prove the fact, and of which the knowledge is so important to the physician, that paralysis of those actions which are dependent on the respiratory nerve of the face, is much more frequently caused by inflammation and suppuration of the parts surrounding the nerve, than by any affection of the brain. The details of those cases would corroborate the observations that have been already made, but they are more adapted to the pages of a journal dedicated to medical science. However, there is one important circumstance in the history of some of those patients, which I should not, on the present occasion, omit. That, after having been repeatedly bled, blistered, and starved, without any change being produced in the symptoms, they have been suddenly cured by the bursting of an abscess, or by the subsiding of an inflammation of the ear. This statement will perhaps prevent others from undergoing a treatment, by which the local disease was not alleviated, but the general health much impaired.

The knowledge of the fact, that paralysis of the face alone, generally depends on a local cause, will also probably afford relief to the minds of those who are affected with such symptoms, and who in consequence of the idea commonly received,

that all kinds of paralysis are produced by an affection of the brain, imagine that the palsy of the face is only the precursor of that more general attack, which will reduce them to the most helpless condition.

Several examples might also be offered, to shew that a knowledge of the functions and of the distribution of the nerves of the head, is not less important to the surgeon in planning his operations for the removal of tumors, *&c.*, from the face, than it is to the physician, in detecting the causes of the different kinds of paralysis.

If a surgeon has an accurate knowledge of the small nerves which pass from before the ear to the mouth and nostril, he may, in extirpating tumors from the cheek, save those branches, and thus avoid substituting distortion, for the deformity which he wished to remove; a result which has hitherto often followed operations on the face, in consequence of our proceeding on the old idea, that although the branches of the portio dura were cut, still the part of the face to which they went, would be sufficiently supplied with nerves from the 5th pair.

I need scarcely add, that if in this inquiry I have been able to note certain important circumstances in the symptoms and causes of various kinds of paralysis, which have been overlooked by the many acute and learned men who have anxiously and carefully investigated the subject, it has been in consequence of my having been directed in my inquiries, by a knowledge of the minute observations on the anatomy of the nervous system, and of the discoveries which have been lately made by Mr. Charles Bell.

The importance of the discussion is sufficiently proved by the circumstance, that not less than eighteen cases of different kinds of paralysis, the causes of which were hitherto but very imperfectly understood, have come within my limited opportunities of observation, in the course of the last twelve months.

Albany, March, 1822. JOHN SHAW.

Art. XII. *A Letter to the Editor, from the* Rev. Edw. Hincks, *respecting Secret Writing.*

Sir,—As a supplement to the paper on secret writing, which you did me the honour to insert in your twenty-third Number, I request you will now publish the following:

I have received a very handsome letter from Mr. Chenevix, enclosing a draft for 100*l*. "A particular motive," he says, "induced me to submit the cipher you have unravelled to a severe examination; and the result has shewn that the mode I pursued was well adapted to the purpose. That cipher, however, is but a small part of a system infinitely more general, but which I delayed mentioning, until the strength of this, its weakest portion, had been tried."

The following is the explanation of the sentences in Mr. Chenevix's paper, numbered 17 and 18:

No. 17.—Although two keys are used here, the secrecy of the method is such, that in another example but one key is used; and no apprehension is entertained that the meaning can be detected.

No. 18.—No cipher in existence, and employing such simple means as the present, could bear so severe a trial as that to which this method is here put; and, should it come off triumphantly, it must be confessed to have fulfilled every desideratum except the least important: it is not quite void of suspicion.

I have the honour to be, your most obedient servant,

Edw. Hincks.

Art. XIII. *A Translation of* Rey's *Essays on the Calcination of Metals*, &c.

[Communicated by John George Children, Esq., F.R.S., *&c.*]

Continued from Vol. XII., p. 299.

ESSAY XVIII.

It is not the consumption of the aërial parts that increases the weight of lead.

Scaliger is so united with Cardan that I cannot separate them; he must follow him here as elsewhere. In his *Exer-*

citation, No. 101, section 18, he supposes that the increased weight of calcined lead is occasioned by its aërial parts being consumed by the fire; for which same reason, he says, "a baked tile weighs more than an unbaked one." Oh! how resemblances often deceive ingenious minds! This great man, seeing the calx of lead and the tile become heavier, after passing through the fire, supposing the effect the same, sought only one same cause. It is however very different. The tile increases in weight by dimunition of bulk, the calx by the matter united with it. To make this more intelligible, who is ignorant that the tile is formed of a fat and sandy earth, kneaded with water, and that the sun, absorbing its moisture, leaves in it an infinite number of little caverns which the water had previously filled? When baked in the furnace, the heat softens it, as it softens metals, and almost brings it into fusion, and in fact does fuse it, if the heat be excessive. In this softening, the parts contract, unite and stick together; the cavities disappear, and diminution of bulk ensues; whence its greater weight, as I have often said before. As for the lead, it fuses in the fire, as is known, and being melted, touches the vessel in all parts, not leaving the smallest portion of air within itself, according to the privilege which nature has bestowed on ponderous and fluid substances, to force upwards those that are less so, always sinking down in them. This lead, set aside, cools by degrees; whilst its parts re-unite, and it congeals, sinking down in itself, and diminishing in volume, as appears by the little hollow, observed on the top when it is cold; so that we cannot suppose any air to be enclosed in this heavy mass. Re-melt and calcine it, and you will find it heavier, not from the consumption of aërial parts, for it had none, but on account of the denser air united to it, as I have already said. And in point of fact, if it lost any aërial parts, would it not diminish in volume? On the contrary, its bulk increases. Besides, if this opinion were correct, why do not stones and plants gain weight by calcination? Hence I infer that the consumption of the aërial parts never increases the weight of substances, when not followed by a diminution of

bulk, which not being the case in the point in question, it cannot be admitted as the cause of the increased weight of which I am speaking. I add in conclusion, that air, forcibly condensed in a balloon full of it, at its escape from it reduces its weight, so far from increasing it, as Scaliger pretends. It is true, this occurs in that instance only.

ESSAY XIX.

It is not soot that increases the weight of the calx.

I read in the 10th chapter of the sixth book of the *Chemical Secrets of Libavius**, (for I have not seen it elsewhere,) that Cæsalpin has written, that it is a thing worthy of admiration that

* "Cum metallum Indicum, quod stannum Indicum et calcem vocant, ignibus exploraremus, id quod prius erat gravissimum, in levissimam favillam, et telas instar aranearum, fuliginemque pompholygi similem redigi mirabamur. Transmutatio nimirum itidem pondus variat, non sola alteratio, qualis videtur esse in laterculis. Hi enim crudi, leviores sunt quam cocti, et tamen perdiderunt humiditatem adherentem ignibus. Huc pertinet et Modestini Fachsii (*a german chemist*) quæstio, qui fiat in examine metallico, res ignem passæ, non sine detrimento favillarum ponderosiores inveniantur, quam nondum illatæ. Si enim testam, plumbum, metallum et cupellam ponderes ante examen, omnia sunt leviora quam post, iterum ad libellam revocatis scoriis, testâ, stanno seu massulâ et cupellâ, cum tamen de plumbo multum perierit. Andreas Cæsalpinus, de metallicis, lib. iii, c. 7, scribit admiratione dignum esse, quod plumbum nigrum ustum in fornace, donec cinis fiat, pondere crescat octo aut decem, pro singulis centenariis, putatque idem fieri quod lateribus in fornace coctis, qui et ipsi post assationem, graviores redduntur, cum oppositum deberet fieri, absumptâ multâ eorum substantiâ in igni. Vult autem in loco deperditæ accedere fuliginem ignis, quæ adhærens lateribus in poris condensetur, quod magis fiat in plumbo, quia in furno reverberii flamma super cineres reflectatur, ibique reponat suam fuliginem: argumento, quod si iterum cinis in plumbum vertatur, moles diminuatur, reliquo abeunte in excrementa; unde concludit, ustione minui plumbum; imbribus augescere. Ex dictis facile respondeas Fachsio evenire quæsitum ob plumbi pondus auctum crematurâ; sed Cæsalpini ratio vel à Tyronibus physicis ridebitur, qui colligent sic etiam calcem vivam debere fieri graviorem, nec posse id fieri à levissimâ ignis fuligine."—Libavii, *Alchymiæ Arcana*, Lib. vi. c. 10. Francofurti, M.DC.LX

common lead*, when calcined, gains eight or ten per cent. in weight. Then, seeking for the cause, he says it is the soot porduced by the fire, which striking against the vault of the reverberatory furnace, falls back on the matter, which Cæsalpin would never have advanced, if he had paid attention to what I am about to remark. First, the soot, as it is gradually exhaled by the fire, is of so thin a nature that the seven ounces of increase found by the Sieur Brun, would occupy more space than all the calx he derived from his calcination. In the second place, the abundance of soot would so blacken the calx of tin and of lead, that the ladies would never whiten their faces with it, as many do. Besides, what should hinder us from increasing the calx *ad infinitum*, since the fire may be kept up as long as we will, and would always furnish soot? Let me add, that the Sieur Brun calcined his tin with a naked open fire, so that the soot could only pass on one side, by the registers of the furnaces, and fly off, and not fall down on the substance, in which too it could not sink, being lighter than the air contained in the vessel. As to Libavius, he rejects the opinion of Cæsalpin, and even says that the very apprentices in chemistry will laugh at it, without, however, having himself adduced much by way of argument against it, (*à l'encontre*,) wrapping up his opinion in such a heap of words, that it is not easy to develope it. Yet he would have such expressions as these be received as a solution of the difficulty. "Transmutation changes the weight," and a few lines further, "Burning increases the weight of the lead." The better to see the force of which answers, we need only slightly vary the terms of our question, (the sense remaining the same,) and apply them to him as thus, Why does the transmutation of lead into calx alter its weight? Because,

* *Plomb noir*, so translated by Cottgrave. The mineral called black lead, plumbago, (carburet of iron,) seems to have been called plumbagine, which Cottgrave says "is pure lead, turned almost into ashes by the vehemence of the fire. This is the artificial plombagine, and comes of lead put into the furnace with gold or silver ore, to make them melt the sooner (by which employment it gains some part in the worth of those metals.) There is also a natural or mineral plombagine, which (as Mathiolus thinketh,) is no other than silver mingled with lead-stone, or ore.'

says he, transmutation does alter it? "Why does burning increase the weight of lead?" Because, says he, burning does increase it. Observe, I pray you, if what he says of Cæsalpin's reasons, be not applicable to his own? Certainly one need not be a great chemist, nor a great logician withal, to laugh at it. He that likes may admit these reasons, for my part I never shall adopt them. But I blush to disclose the disgrace of this person, who otherwise deserves great praise for the number of works he has published, full of much learning.

ESSAY XX.

The increased weight of the calx of tin, or lead, is not derived from the vessel.

I now come to opinions which, as far as I know, have not been committed to writing. As nothing is in contact with the tin and lead during calcination, except the air and the vessel, they who will not acknowledge the former as the cause of its increased weight, cannot, I think, have recourse, with any seeming to aught but the latter. For they might persuade themselves that, during the calcination and continual stirring of the aforesaid metals, the iron by burning becomes friable at its surface, a portion of which might mix with the calx, and so increase its weight; just as pearls, which the apothecary pounds in his marble mortar, acquire weight by the addition of the stony matter which, crumbling off, mixes with them, often to their prejudice to whom they are afterwards disposed of. But to do away this opinion, I observe, first, that if the powdered iron, which is brown, were to mix in such large quantity with the tin, it would render its calx dark coloured, which nevertheless is always white. Secondly, if the vessel were thus consumed, in two or three calcinations at most it would become useless; whereas, it lasts many years, though used every day. Thirdly, from a very little tin or lead we should obtain abundance of calx, the whole vessel being easily reduced to powder by continuing the fire, which experience contradicts. What is more, Modestinus Fachsius has observed, (as Libavius relates in the

place alluded to above,) that in the examination of metals the vessel, the cupel, the lead and the metal examined, are altogether heavier after the process than before they had undergone the fire, notwithstanding the loss of much matter that flies off in fumes, which could not happen but from a great deal of the above-mentioned air adhering to the whole, a thing which till now has not been clearly understood. Thus the calcining vessel itself may become heavier, which I beg the Sieur Brunn to take notice of.

ESSAY XXI.

The vapours of the charcoal do not increase the weight.

I have been told, (if truly I know not,) that one of my intimate friends *, a man of profound knowledge, and the most polished and solid judgment, to whom the Sieur Brun made the same request as to me, suffered himself to believe that the increased weight in question proceeds from the vapours of the charcoal, which, passing through the vessel, mix amongst the calx, which I maintain is impossible. For if such vapours cannot pass through a glass phial, a tin plate, an earthen pot, (otherwise our boiled waters, our sauces, our pottage, would be infected with it,) how can they pass through a vessel of iron, whose material is so much stronger? If the most subtle air cannot penetrate it, (what otherwise would my Æolipile be worth?) how shall these grosser vapours? But if they could penetrate it, what shackles would they find in the calx to retain them? Why should they finish their course there? The heat, by its violence, expels from the tin and the lead the moisture which connected their parts, driving far away all the metallic vapours, although natural to them; and it will leave these foreign vapours! There is no probability in it. Oh! truth, how dear art thou to me, who canst make me contend against so dear a friend †!

[To be continued.]

* *Deschamps*, according to M. Gobet, who says he was a doctor of physic at Bergerac, a skilful mathematician, and a pupil of the celebrated Rodolphus Snellius, professor at Leyden, in 1609.

† Honest John Rey might fairly say with the philosopher of old, "Amicus Plato, amicus Socrates, sed magis amica veritas;" an admirable motto for all who cultivate Science as she should be cultivated, not to support a system or a sect, but to develope truth.

Art. XIV. *Proceedings of the Royal Society.*

The following papers have been read at the table of the Royal Society since our last report.

Jan. 10. Extract of a letter from Capt. Basil Hall, R.N. to Dr. W. H. Wollaston, containing observations on a comet seen at Valparaiso.

Elements of Capt. Hall's comet, in a letter from Dr. Brinkley to Dr. Wollaston.

17. On the ultimate atoms of the atmosphere, by William Hyde Wollaston, M.D. V.P.R.S.

On the expansion in a series of attraction in a spheroid, by James Ivory, Esq., A.M. F.R.S.

24. On the late extraordinary depression of the barometer, by Luke Howard, Esq., F.R.S.

On the anomalous magnetic action of hot iron, between the white and blood-red heat, by Peter Barlow, Esq.; communicated by Major Colby, F.R.S.

31. Observations for ascertaining the length of the pendulum at Madras, by John Goldingham, Esq., F.R.S.

Feb. 7, 14, and 21. An account of an assemblage of fossil teeth and bones, discovered in a cave at Kirkdale, near Kirby-Moorside, Yorkshire, by the Rev. William Buckland, Prof. Min. and Geol. Oxon., F.R.S.

28. Communication of a curious appearance lately observed upon the Moon, by the Rev. C. F. Fallows, F.R.S.

On the difference in the appearance of the teeth, and shape of the skull, in different species of seals, by Sir Everard Home, Bart. V.P.R.S.

March 7. Experiments and observations on the developement of magnetical properties in steel and iron, by percussion, by W. Scoresby, jun., Esq.; communicated by the President.

14 and 21. On the alloys of steel, by Messrs. Stodart and Faraday; communicated by J. Stodart, Esq., F.R.S.

Art. XV. PROGRESS OF FOREIGN SCIENCE.

I. Chemical Science.

I. *Principles of Combination.* In Sir H. Davy's researches "on some chemical agencies of electricity," published in the *Phil. Trans.* for 1807, we find it stated, that "when oxalic, succinic, benzoic, or boracic acid, perfectly dry, either in powder or crystals, was touched upon an extended surface, with a plate of copper insulated with a glass handle, the copper was found positive, the acid negative. In favourable weather, and when the electrometer was in perfect condition, one contact of the metal was sufficient to produce a sensible charge; but seldom more than five or six were required. Other metals, zinc and tin, for instance, were tried with the same effect. And the metal received the positive charge, apparently to the same extent, whether the acid was insulated upon glass, or connected with the ground. The solid acid of phosphorus, which had been strongly ignited, and most carefully excluded from the contact of air, rendered the insulated plate of zinc positive by four contacts, but after exposure to the atmosphere for a few minutes, it wholly lost this power. When metallic plates were made to touch dry lime, strontites, or magnesia, the metal became negative: the effect was exceedingly distinct, a single contact upon a large surface being sufficient to communicate a considerable charge. For these experiments the earths were carefully prepared; they were in powder, and had been kept for several days in glass bottles before they were used. *It is essential to the success of the process, that they be of the temperature of the atmosphere.* In some experiments which I made upon them, when cooling after having been ignited, they appeared strongly electrical, and rendered the conductors brought in contact with them positive."—"Supposing two bodies, the particles of which are in different electrical states, and those states sufficiently exalted to give them an attractive force superior to the power of aggregation, a combination would take place, which would be more or less intense, according as the energies are more or less perfectly balanced; and the change of properties would be correspondingly proportional. This would be the simplest case of chemical union. But different substances have different degrees of the same electrical energy in relation to the same body; thus the different acids and alkalis are possessed of different energies with regard to the same metal," *&c.*

These experiments, and views relative to electro-chemical action, have met with universal admiration and general acquiescence from the chemical world. The electroscope was that of Bennet, furnished with small condensing plates.

Dr. Brewster has published, in the last number of his Journal, "Observations on the Production of Electricity by Contact, by Professor Gmelin of Tübingen, in which we find several experiments, made with the aid of Bohnenberger's new electrometer, with two zambonic piles, that seem adverse to the above results of the English philosopher. Of one experiment with magnesia, the Professor says, "This experiment, which was very often repeated, appeared to refute the *experiments* of Sir H. Davy so fully, that they scarcely seemed to require any further refutation." And he concludes as follows:—"The general result of these experiments is therefore this, that the electrical opposition between acids and bases, though so well established by other means, *cannot* be deduced from the electrical relation between these bodies and metals."

Most scientific readers, we believe, will hesitate a little before they give up Sir H. Davy's results, obtained by a simple instrument, which, by the way, Professor Gmelin disparages, for others of an anomalous character, inconsistent with themselves, and with the well-known indications of voltaic electricity.

II. Caloric. We promised in our last to give some account of M. Fourier's speculations on the temperature of the earth. According to Mr. Fox, the temperature of metallic veins is generally from one to three centigrade degrees above that of the contiguous rocks. Considering the currents of air which circulate in the galleries of mines, as a constant cooling cause, it would appear that the above small difference of temperature should be the necessary consequence of the great conductibility of metals. It appears, also, that, this phenomenon properly studied, might serve to shew that the *interior* parts of rocks and metallic veins are themselves at still more elevated temperatures, and that it would thus furnish a new objection against the hypothesis advanced by some philosophers, that the high heat observed in mines proceeds from a chemical action of the air on the substances which line the walls of the galleries, and, in particular, the pyrites. These ideas of M. Arago seem confirmed by M. Fourier's investigations.

1. Suppose that a homogeneous solid mass is enclosed between two infinite parallel planes, and that one of these extreme planes, being exposed to the action of a constant source of heat, is maintained at the fixed temperature b, whilst the opposite plane, which terminates the solid, is in contact with the atmospherical air standing at a fixed temperature a, less than b. If we conceive that the solid has been subjected for a very long time to these unequal but constant actions, of two exterior causes, the surface in contact with the air will take a

fixed temperature Q less than b, and greater than a; now, the value of Q is given by the following equation.

$$Q - a = \frac{b - a}{1 + e^{\frac{h}{k}}}$$

By e we denote the depth of the solid, or the distance between the two extreme planes; k and h are specific coefficients, that is to say, peculiar to the substance of which the solid is formed. These coefficients must be determined by observations. The first k measures the facility with which the heat travels in this substance, from one particle to another. The second h measures the facility with which the heat is dissipated by the surface. This effect includes two modes of cooling, namely, that due to radiation, and that occasioned by the contact of the air. If, for different substances, the coefficient h is the same, or little different, and if the temperatures b and a, and the depth e, are the same, the difference Q — a will be greater when the conductibility is greater.

2. Suppose that a homogeneous solid mass is terminated by an infinite plane, and that the depth of the mass below this plane is extremely great. Let us assign to every point of the mass a common initial temperature, b, and let the plane which terminates it remain exposed to the air, kept at the fixed temperature zero. The initial temperature b decreases slowly, in proportion as the heat is dissipated in the air. After a given time, marked t, the temperature Q of the surface has a certain value expressed in a function of t, and of the specific coefficients h and k, which regulate the motion of the heat, in the substance of which the solid is formed. It is required to know how far this decreasing temperature Q depends on the peculiar conductibility of the solid. M. Fourier gives an equation of some intricacy, from which he infers that the temperature Q of the surface is so much greater, as the heat is more easily conducted in the interior of the solid; all other conditions being supposed the same *.

Under the head of CALORIC may be ranged *phosphorescent phenomena.* A mixture of antimony and cream of tartar, exposed to the action of fire in a close crucible, leaves a residuum, which is an alloy of potassium and antimony. This forms, in a certain state, a very active pyrophorus. Tartar emetic in powder, enclosed in a covered crucible, when ignited for a couple of hours, presents after cooling a carbonaceous-looking mass, mammelated above like cauliflower, and interiorly radiated like antimony. This mass inflames suddenly in the air, while the revived antimony runs together into globules of extreme brilliancy,

* *Ann. de Chimie et de Physique*, xvi. 82.

which then oxidize on their surface. This phenomenon was noticed first by Klaproth. Mr. Serullas states that the great inflammability of this alloy renders it difficult to transfer it from a crucible into a phial; and even dangerous, for, on breaking the mass with an iron rod, the whole is apt to explode with great violence. He accordingly prefers the following process. He introduces into a luted phial the tartar emetic, closes it with a stopper of chalk, and heats it moderately till the flame from the combustion of the carburetted hydrogen has ceased to appear round the stopper. He then withdraws the phial, breaks it when it is cool, pulverizes its contents, which he puts into another phial, the chalk stopper of which he lutes with a mixture of sand and clay, and exposes it to a stronger heat than in the first operation. When the flame of the oxide of carbon disappears, the process is completed. After the phial becomes cool, a cork replaces the chalk stopper *.

Phosphorescence of Sulphate of Quinina.—It appears from the observations of M. Callaude, apothecary at Annecy, that sulphate of quinina, exposed to a gentle heat, becomes highly luminous. M. Pelletier has since found, that sulphate of cinchonina, as well as these two salts mixed, when exposed in a capsule to the steam of boiling water, becomes luminous; but that neither quinina or cinchonina by themselves, nor their acetates, possess the phosphorescent quality †.

III. We have not perceived since our last, in the foreign department, any thing of interest on the class of Simple Bodies. Nor can we observe many important new facts on the class of Compounds. M. Vogel read to the Royal Academy of Sciences at Munich, a memoir on the action of sulphuric acid on the muriates, from which we have extracted the following particulars. When we pour concentrated sulphuric acid on the hydrated deuto-muriate of copper, this salt loses instantly its green colour, and becomes brown, because the acid has abstracted its water; but no muriatic acid gas is disengaged, nor do we perceive any sign of effervescence. After preserving the mixture for some days in a close vessel, we can decant off the sulphuric acid, which is perfectly colourless, and does not contain a trace of copper, the muriate of copper remaining quite undecomposed at the bottom of the vessel. On exposing this salt to moist air, or on sprinkling on it a few drops of water, it resumes its green colour. On heating the above mixture, muriatic acid gas is readily disengaged, and sulphate of copper is formed. Calomel is not affected in the cold by sulphuric acid, but at a boil-

† *Journ. de Pharm.* Sept. 1821.
‡ *Journ. de Pharm.* Dec. 1821.

ing heat, corrosive sublimate and deuto-sulphate of mercury are formed. No muriatic gas is evolved. On heating sulphur and calomel together, he obtained muriatic acid gas, indicating, of course, the presence of hydrogen in the former body. The two sublimed muriates of antimony and bismuth in a concrete state, are not affected at ordinary temperatures by sulphuric acid. No action occurs between muriate of silver and sulphuric acid without heat. M. Vogel says, that it is only the muriates, whose metals are capable of decomposing water, such as iron, zinc, and manganese, which occasion an effervescence in the cold with sulphuric acid, and that all the muriates whose bases do not decompose water, make no effervescence with sulphuric acid without the aid of heat.

IV. We promised in the last Number to notice M. Robiquet's speculations on the constitution of *ferro-prussiate of potash.* "Mr. Porrett," says he, "after having varied much with regard to the composition of this salt, regarded it in his last analysis, as formed by the union of one atom of prussic acid, two atoms of carbon, and one atom of iron; whilst, according to my experiments, it should be considered as a combination containing the elements of the hydrocyanic acid, and of the cyanide of iron, just as we conceive alkohol to be formed of water and olefiant gas. I had inferred, as a consequence of this manner of viewing the acid, that the triple prussiates might themselves be considered as a double combination of a hydrocyanate, differing for the different prussiates, and of a cyanide of iron, constant for the whole. M. Berzelius has just published a very extensive memoir, in which he labours especially to disprove the existence of this acid. The very numerous analytical experiments which he has made, to determine the composition of the triple prussiates, have led him to admit that they were all formed of one atom of cyanide of iron, and of two atoms of the cyanide of another metal. The main purpose of M. Berzelius is to establish, that the acid product which Mr. Porrett and I obtained, in decomposing the triple prussiates, is not a peculiar acid, but merely an acidulous hydrocyanate of the protoxide of iron; so that this hydrocyanate, in combining with another base, forms a double hydrocyanate susceptible of conversion, by mere drying, into a double cyanide." It is known that in Prussian blue, the portion of iron which supplants the variable base (potassium or potash, for example), is in the state of peroxide. If this combination be put in contact for a sufficient time with sulphuretted hydrogen water, there is a partial deoxidizement of the peroxide, and this retrogradation cannot take place without a proportionate diminution of its capacity of saturation, and of consequence also, without a proportionate quantity of the acid being set at liberty. If M.

Berzelius' view be correct, it is merely hydrocyanic acid which should be liberated in this case; whilst on the contrary, M. Robiquet finds that it is the ferro-cyanic acid of Mr. Porrett. M. Robiquet conceives, on mixing a solution of a neutral salt of black oxide of iron with one of triple prussiate of potash, in the fabrication of Prussian blue, that till the iron passes to the *maximum* of oxidizement, the precipitate always retains a proportional quantity of alcali, which maintains the neutrality of the precipitate; and, that the perfection of Prussian blue consists chiefly in the complete peroxidation of the iron, and in the total abstraction of the potash; a result which can be obtained only by the joint agency of oxygen and an acid. Acting otherwise we do not obtain Prussian blue properly so called; so much so, that that of M. Berzelius formed with a solution of black oxide of iron, and simple washing with distilled water, did not acquire, notwithstanding its exposure to the atmospheric air, the qualities of ordinary Prussian blue. "The Prussian blue thus prepared," says Berzelius, "possesses properties which it does not do, when it is otherwise prepared; it is soluble in pure water," &c.

M. Robiquet hence thinks that M. Berzelius is mistaken in regarding Prussian blue as a subsalt, or at least as containing an excess of oxide. Besides, how can we admit that Prussian blue contains an excess of oxide, since, when it is treated by the most energetic and concentrated acids, we precipitate it entirely, and without abstraction of any portion of oxide, by the mere addition of distilled water. If after triturating Prussian blue along with concentrated sulphuric acid, we leave it to settle, and then decant the supernatant acid, we shall not find in it any trace of iron. This is not, however, the case with muriatic acid; but here the residuum contains an excess of ferro-cyanic acid, since it was by this process that M. Robiquet succeeded in separating that acid from Prussian blue. Nor are the results obtained by M. Berzelius incompatible with the existence of this acid. In fact, when it combines with a base, the oxygen of the latter may be conceived to unite with the hydrogen of the acid; and the abstraction of these two elements by evaporation, would leave a compound represented by the constituents of cyanide of iron, and those of the other base, so that the analysis of this combination would present merely cyanogen and the metallic radicals. This transformation of the ferro-cyanates into double cyanides, does not take place undoubtedly for all the bases, and Prussian blue is itself a striking example of it, since however it be desiccated, it exhibits always hydrogenated products by its calcination in close vessels.

M. Robiquet agrees with M. Berzelius with regard to the proportion of water, potash, and iron, in the triple prussiate;

but he considers the black flocks, which are obtained on washing the residuum of the calcination of that salt in an earthen retort, as not being a quadri-carburet of iron, but a mere mixture of the two ingredients. Pure silica ignited in an earthen retort with anhydrous ferro-prussiate of potash, after yielding an immense quantity of gases, chiefly azote, left a siliceous combination with the residuum of the triple salt, which was partly a simple hydro-cyanate of potash undecomposed. How can we imagine this compound with silica to be formed, if potassium alone be present; or how does not the silica expel the cyanogen from its combination?

When the anhydrous ferro-prussiate mixed with fused boracic acid was exposed to ignition in a glass tube, the apparatus always burst from the frothing up of the materials. With regard to the gaseous products of this salt by the action of ignited oxide of copper, the results have differed widely. Mr. Porrett found 4 of carbonic acid to 1 of azote; M. Berzelius obtained the proportion of 3 to 2; and M. Robiquet that of 2 to 1. These differences seem to proceed from the mode of operating, and the period of the process at which the examination is made. If the heat be applied slowly and successively, a pretty steady proportion is obtained during the whole course of the operation; but a part of the carbonic acid is retained by the potash. When the heat is applied suddenly over the whole extent of the tube, the ratio of the carbonic acid to the azote is extremely variable. No precise analytical results can be inferred from the experiments with peroxide of copper, for the above reasons. But M. Robiquet has judiciously combined the two actions of oxide of copper and boracic acid on the anhydrous prussiate. This experiment completely succeeded. He mixed together 1 gramme of anhydrous prussiate, 2 gr. of fused boracic acid, and 20 gr. of oxide of copper. The whole was introduced into a strong tube of small diameter. To this tube he joined a second, containing muriate of lime exactly weighed, from which a third, conducted the gases under the glass receiver. After making the requisite reductions in the gaseous products, he had obtained:

$$\left.\begin{array}{ll} \text{Azote} & 0.2297 \\ \text{Carbon} & 0.1994 \end{array}\right\} \text{or Cyanogen } 0.4291$$

a quantity which, combined with the proportions of iron and potash found in one of his preceding experiments, gives for one grain of prussiate,

$$\left.\begin{array}{ll} \text{Cyanogen} & 0.4291 \\ \text{Iron} \quad . & 0.1442 \\ \text{Potash} \quad . & 0.4980 \end{array}\right\} \text{forming a sum too great by } 0.0713.$$

$$\overline{1.0713}$$

But in admitting, with M. Berzelius, that the anhydrous prussiate contains potassium, we have,

Cyanogen	0.4291
Iron	0.1442
Potassium . . .	0.415
	0.9883

a result as near as possible to the weight employed. He proved by a subsequent experiment with oxide of copper and fused boracic acid, heated together, that the water which appeared in decomposing the triple prussiate, was derived from the boracic acid, for the quantity of moisture absorbed by the muriate of lime was the same in both experiments, amounting to 0.45 in the first; surely much greater than should exist in 2 grains of well ignited boracic acid.

V. Organic Compounds.—*Analysis of the roots of black hellebore*, by MM. Feneulle and Capron MM. Pelletier and Caventou some time since analyzed the roots of white hellebore, and M. Vauquelin those of the *helleborus hyemalis*. The latter chemist found that the activity of the plant resides in a principle contained in the oil. The present analyzers agree with him in referring the activity of black hellebore to no principle of an alcaline nature. The final result of their examination gave the following products:

1. A volatile oil; 2. a fat matter; 3. a resinous matter; 4. wax; 5. a volatile acid; 6. a bitter principle; 7. mucus; 8. alumina; 9. gallate of potash, and acidulous gallate of lime; 10. a salt, with an ammoniacal basis*.

M. F. Cartier has lately analyzed the petals of the white provençal rose, from 1000 grammes of which he obtained, after incineration, a residuum of 99 gr., containing subcarbonate of potash, phosphate, and traces of muriate of the same base, carbonate of lime, phosphate of lime, traces of phosphate of magnesia, some silica and oxide of iron. The quantity of the last substance was determined with precision; it was 12.5. From 1000 gr. of red roses, he procured only 50 of residuum, which contained no more than 4 oxide of iron. He says that a physician of Provins had some time ago hazarded the opinion that roses contained iron†.

The ichorous secretion of the *tinea capitis*, consists, according to M. Morin's analysis, of superacetate of ammonia, osmazome, gelatine, fluid albumen, concrete albumen in great abundance, a fatty matter, chloride of sodium, with traces of phosphate and sulphate of lime; in all nine ingredients ‡.

M. Bouillon Lagrange has extracted from the *semen contra*

* *Jour. de Phar.*, Nov. 1821. † *Ibid.* ‡ *Ibid.*

vermes, an essential oil, which he advises to be given in small quantities, mixed with syrup, as a valuable anthelmintic. The above substance, is not, as some authors have stated, the seed of a plant sent from Persia, but the flower-bud of the *artemisia contra L.*

In the *Journal de Pharmacie* for Dec. 1821, we find a letter from Mr. Thos. Morson, an English apothecary, dated London, 12th Nov., to M. Planche, one of the editors ofthat work. He says, "The important discovery of cinchonina will enable us to discover certain sophistications used in commerce. From peculiar circumstances I am in possession of the following formula for preparing extract of cinchona: Take of the extract of the horse chestnut-tree bark 200 p., yellow rosin 25 p.; mingle. A very large quantity of this strange composition has recently been sold to the apothecaries in the different parts of England, and on the continent." The respectable druggists and apothecaries of London, who supply this kingdom, too well know the pharmaceutical establishments where the extract of Peruvian bark is made; they are above such a fraud.

Under the chemistry of organic bodies we may consider vegetable and animal physiology. Two papers of considerable interest have lately appeared on these subjects. The first is a memoir "On the influence of green fruits on the air, before their maturity, by M. Th. de Saussure," distinguished by the precision of experiment and soundness of induction, characteristic of this philosopher's researches. It was read before the Physical Society of Geneva, 7th Sept., 1821, and subverts *de fond en comble*, the dissertation of M. Berard, *crowned* by the Institute of France, of which some account was given in the 11th vol. of this Journal, p. 395.

Researches elicited by the specific stimulus of money or medallions, have not of late years, it must be allowed, aided much the promotion of science. Like the prize poetry of our schools, these forced plants of philosophy, however gaudy their foliage and inflorescence, seldom bear any solid fruit, while they too often poison the surrounding air with rank and jealous exhalations. As the passion for theatrical effect, will render such academic games always agreeable to our lively neighbours, care should be taken that the unsuccessful competitors may not have it in their power to accuse the arbiters of incapacity, favouritism, or corruption. We know that the decision in favour of M. Berard excited much dissatisfaction, and some surprise, in Paris, and that it was ascribed by many to the protection of a powerful patron. Among the luckless candidates there was one of no mean talent, who took the opposite view of the subject to M. Berard, a view in unison with M. de Saussure's early researches

on vegetation, which is now fully confirmed by his recent experiments.

"When I was occupied," says this able philosopher, "in my *researches on vegetation**, with the action of green fruits on the atmospheric air, I admitted that they produced the same effects as the leaves, or that they poured out like them oxygenous gas, by the decomposition of the carbonic acid, with this difference that, in an equal volume, they decomposed much less. My experiments on this subject indicate, that grapes in the state of verjuice, and the green fruit of the *solanum pseudocapsicum*, exposed to the sun, and adhering to the plant and soil, which make them grow, add oxygen to the air contained in the vessel in which they are enclosed; whilst the same fruits, in circumstances otherwise equal, destroy the oxygen of the vessel, when it contains hydrate of lime. This substance, by absorbing the carbonic acid which they form, and that which they receive from the soil, prevents the oxygen from making its appearance, which would otherwise have been disengaged.

"In the experiments which I published, the disengagement of oxygen gas had not the same success when the fruits were detached from the plant that bore them. Like the leaves, they absorbed oxygen gas from the air in obscurity, replacing it in a volume nearly equal to the fruit, by an equal quantity of carbonic acid gas; but in the sun they decomposed only in part the acid gas produced during the night, whilst on the plant they decomposed it altogether. This partial and purely accidental difference depended evidently on the decay or loss of the vegetative force which a fruit must suffer when it is detached from its plant, and which receives no nourishment; and this difference ought not to affect the experiments which led to the conclusion that green fruits comport themselves in the air, as the leaves do. These experiments offered, moreover, merely a confirmation of the principle which supposes that the faculty of emitting oxygen gas in the sun is essential to the green herbaceous parts in a state of vegetation."

M. Berard deduced from his experiments the remarkable result, that green fruits, in any period of their growth, *do not* comport themselves like leaves in sunshine; that they do not there decompose the carbonic acid gas; that they do not there disengage oxygen gas, and that the only action which they exercise on the atmosphere in every period of their vegetation, is to transform its oxygen into carbonic acid; he is even led to believe that, in an equal time, green fruits cause the disappearance of more oxygen in the sun than in the shade.

M. de Saussure justly observes, that since the object is to

* Pages 57 and 129.

ascertain if the green herbaceous substance of fruits, considered in itself, disengages oxygen gas, we ought to consider that those in which this colour is very feeble, and which are formed of a very thick, yellow or white, parenchyma, cannot lead to any very determinate result; for it is admitted, (with very rare exceptions,) that the vegetable matters, which are not green, corrupt the air, both in the sun and the shade, in whatever part they are found, and that their effect may overbalance that of the green portions. We regret that our bounds will not permit us to follow the Genevese philosopher through the details of his masterly and decisive researches. The following is his recapitulation of results, on which, we conceive, perfect reliance may be placed.

1. Green fruits have the same influence on the air, in sunshine and obscurity, as the leaves; the action differs only in being less intense than that of the latter.

2. They cause the disappearance, during the night, of the oxygen gas of their atmosphere, and they replace it by carbonic acid gas, which they partially absorb; this absorption is usually less in the open air than under a receiver.

3. In equal volume, they consume more oxygen in the dark, when they are distant from, than when they are near to, their maturity.

4. During their exposure to the sun, they disengage, in whole or in part, the oxygen of the carbonic acid which they have imbibed during the night, and leave no trace of this acid in their atmosphere. Several fruits detached from the plant, add also oxygen gas to air which contained no carbonic acid. When their vegetation is very feeble or languishing, they corrupt the air in every circumstance, but less in the sun than in the shade.

5. Green fruits detached from the plant, and exposed to the successive action of the night and of the sun, change the air little or nothing in purity and volume; the slight variations, which we observe in this respect, depend either on their greater or less faculty of elaborating carbonic acid, or on their composition, which is modified according to their degree of maturity.

6. Green fruits decompose, in whole or in part, not only the carbonic acid which they have produced during the night, but also that which we add artificially to their atmosphere. When we make the latter experiment, with fruits which are aqueous, and which, such as apples and grapes, elaborate but slowly the carbonic acid gas, we perceive that they absorb in the sun, a portion of gas much greater than a like volume of water could do in a similar mixture. Thereafter they disengage the oxygen of the absorbed acid, and appear thus to elaborate it in their interior.

7. Their faculty of decomposing carbonic acid, becomes feebler, as they ripen.

8. They appropriate, during their vegetation, the oxygen and hydrogen of the water, making it lose the liquid state.

9. These results are sometimes to be observed only in volumes of air, which exceed 30 or 40 times the volume of the fruit, and by weakening much the heating action of the sun. If we neglect these precautions, several fruits corrupt the air, even in the sun, by forming carbonic acid with the ambient oxygen. But still in the latter case, the mere comparison of their effect in the shade, with that which they produce, under the successive influence of the night and of the sun, demonstrates that they decompose the carbonic acid.

10. The differences between the results of M. Berard and mine, proceed principally from his enclosing the fruits in a space which was only 6 or 8 times greater than their own volume, and which was too narrow for them not to suffer by the neighbourhood or contact of the sides of the receiver heated by the sun. Some succulent plants, *(plantes grasses,)* resist this trial, and my results with the *cactus*, may have induced this chemist to treat fruits by the same process; but several of them require more tender management, not only than the fleshy plants, but even than the most delicate leaves. I think also that he ought to have nourished the fruits with a small quantity of water; the appearance of freshness which he found in them after the experiment, might have some weight, if he had been operating on leaves, which lose their look and consistence by the least drying; but it has little value for thick and fleshy fruits, which may be deteriorated and lose their weight, without giving any such indication by inspection alone.

The second paper to which we referred above, is entitled, " Examination of the Blood, and its Action in the different Phenomena of Life, by J. L. Prevost, M.D., and J. A. Dumas," published in the *Bibliothèque Universelle.* " If we submit," say these gentlemen, " to the action of the voltaic pile, the white of an egg, it is decomposed, the coagulated albumen is carried to the positive pole, the caustic soda to the negative. This experiment, which we owe to Mr. Brande, demonstrates that the white of egg ought to be regarded as an albumenate of soda, with excess of base. We have submitted to a very careful microscopic examination, the coagulum which is produced in these circumstances, and it is not without satisfaction that we have seen very distinct globules similar in every thing to those of the blood, &c.; the same appearance, the same diameter, the same disposition to form ranges or aggregates. This remarkable result appears proper to throw some light on the animal secretions, and in particular on the formation of the chyle." They measured the dimensions of the globules of

blood in different animals, by the method of Captain Kater. The following is their table of results.

Name of the Animal.	Real diameter in parts of an English Inch.	Name of the Animal.	Real diameter in parts of an English Inch.
Callitriche d' Afr	$\frac{1}{3000}$	Grey Mouse	$\frac{1}{4275}$
Man	$\frac{1}{3750}$	White Mouse	do.
Dog	do.	Sheep	$\frac{1}{5000}$
Rabbit	do.	Horse	do.
Pig	do.	Mule	do.
Hedgehog	do.	Ox	do.
Guinea pig	do.	Chamois	$\frac{1}{5450}$
Muscardin	do.	Stag	do.
Ass	$\frac{1}{4175}$	She goat	$\frac{1}{7200}$
Cat	$\frac{1}{4275}$		

According to Sir E. Home and Captain Kater, the mean dimension of the human globule is only $\frac{1}{5000}$ of an inch, which agrees perfectly with the determinations obtained by Dr. Wollaston with his most ingenious micrometer, described in the *Philosophical Transactions for* 1813. Dr. Young, by the employment of his eriometer, has also furnished various measures which may be depended on. The following are extracted from his *Introduction to Medical Literature*, published in 1813.

Diameter of a globule of the blood of the calf	$\frac{1}{6000}$ of an inch.
— Human blood diluted with water	$\frac{1}{6000}$
— Human blood, after being several days in water	$\frac{1}{5000}$
— Blood of the mouse	$\frac{1}{4620}$
— Blood of the thornback	$\frac{1}{1900}$

We thus perceive that the measurements of the foreign observers are, probably, all too high rated. But the most remarkable result which they have arrived at, is that while the globules are circular in all the mammiferæ, varying only in size from one animal to another; that they are elliptical in birds, and vary little in size, the variation being only in the greater axis. They are also elliptical in all animals with cold blood. Another very curious fact is, that the colourless globule which exists in the centre of the particles of blood has exactly the same diameter ($\frac{1}{7500}$) of an inch, whatever be the form of the particle and the animal to which it belongs. In conclusion of their memoir we find some interesting remarks on the transfusion of blood. They bled animals to *syncope*, so that when left alone, or when water or serum of blood at 100° Fahr. was injected into their veins, they died. Into others, exhausted by a similar hæmorrhagy, when they injected the blood of an animal of the same species, every portion of the blood thrown in, re-animated perceptibly this kind of corpse; and they found, not without astonishment, that after restoring

to it a quantity equal to what it had lost, it began to respire freely, to move with ease, to take nourishment, and to be restored to perfect health, when the operation was properly conducted.

If we take the blood to be injected from an animal of a different species, but whose globules have the same form, although of different dimensions, the animal is but imperfectly relieved, and can rarely be kept alive for more than six days. The pulse becomes quicker, the breathing preserves its regular state, but the animal heat falls with a remarkable rapidity when it is not artificially kept up. From the instant of the operation the stools become mucous and bloody, and preserve this character till death. If we inject blood with circular globules into a bird, the animal usually dies in the midst of very violent nervous affections, which may be compared in rapidity, to those caused by the most intense poisons. They are manifested even when the subject on which we operate, has not been enfeebled by any notable loss of blood.

The blood of the cow and the sheep was transfused into cats and rabbits. Whether the operation was practised immediately after the extraction of the blood, or whether this was left at rest in a cool place for 12 or even 24 hours, the animal has been restored for some days, in a great number of cases. The blood was kept in a fluid state, either by eliminating a certain quantity of fibrine, or by adding 0.001 of caustic soda. The blood of the sheep excites in ducks rapid and powerful convulsions, followed by death. They have frequently seen a bird die before they had completely emptied the first syringe, although it had experienced but a slight bloodletting before, and although it was in vigorous health. They consider transfusion in man to be absurd and dangerous, till we be further advanced in the intimate knowledge of the active principle of the blood.

VI. Analysis and Chemical Apparatus.—M. Berzelius, in his elaborate memoir on the manner of analyzing the ores of nickel, inserted in the *Annales de Chimie et de Physique* for last June, after applying to a new native combination of that metal with arsenic, sulphur, and iron, the method by nitric acid; by nitro-muriatic acid; by nitro-muriatic acid and acetate of lead; finally, hits on a more satisfactory mode of analysis by the agency of chlorine, which merits to be generally known. We have alluded to it in our last Number, p. 330, where something similar is described from Gilbert's *Annalen*, as the contrivance of M. Dobereiner.

On a barometer tube, at the distance of three inches from one of its ends, blow a ball of such size as that it will be filled only one-third, by the powder of the substance which we wish

to examine. On the other side of this ball, draw out the tube a little, and blow in it a second ball, somewhat smaller, after which bend the drawn part of the tube, in the manner shewn in the figure D E F G H, (fig. 1). We must weigh the tube first empty, and then with the substance to be analyzed, to learn the weight of the latter.

For disengaging the chlorine, we may employ a vessel, A, which has the capacity of 1, or at most 2 litres, (from 2 to 4 pints). We introduce into it a mixture of common salt and oxide of manganese, and fill the vessel up two-thirds with water. The orifice must then be closed with a cork, through which pass an elongated funnel, B, and a small bent tube, which gives issue to the gas. Fig. A B D, explains this arrangement better than any description. From the bent tube the gas passes into another tube, C, which contains small fragments of fused muriate of lime, and from this tube it enters the small apparatus that contains the powder to be analyzed. The joinings are made by small tubes of caoutchouc firmly tied round the glass tubes. The drawn-out tube, G H, descends perpendicularly into a bottle, H I, two thirds filled with distilled water; G H, passes through a cork which closes the mouth of the bottle, and which contains also another tube, from 24 to 36 inches long, through which the excess of chlorine escapes out of the apartment by a window, or chimney. This arrangement is shewn in the figure G H I K. The bottle can be placed at a convenient height, by means of the screw, M.

When every thing is thus arranged, we pour concentrated sulphuric acid, by the funnel B, into the flask, till a disengagement of gas begins to appear. We must, however, take care that the mixture does not heat too much, which would cause too rapid an extrication of gas. The disengagement of this is sufficiently quick, when four or five bubbles rise every minute into the bottle, H I. As soon as the greater part of the atmospheric air is displaced by the chlorine, we set a spirit of wine lamp at a little distance below the ball, E. We require but a very small flame, and we must avoid too great a heat; for it is difficult to expel entirely, especially at the commencement, the atmospheric air, by which a small part of the ore might produce arsenious acid, which would render the result inexact. According as the mass becomes hot, we perceive an orange-coloured liquid distil over, which condenses in the little ball, F; and as this fills, it runs off by the tube G H, and falls into the water.

During the operation there is formed muriate of peroxide of iron (bichloride), which sublimes in red and transparent spangles, and of which a small quantity is deposited even in the tube E D. For this reason, it is proper to have the tube so long, that the sublimed part may not pass off. Another part of

the muriate is carried by the current in the direction E F. When the acids are condensed with the muriate, there results a white and crystalline matter, a little of which descends even to the small ball F, which was made to hinder the muriate of iron from falling into the bottle. This white mass is insoluble in the orange liquid. After draining off the latter into the bottle, we may decompose the white mass by a gentle heat, the double acids (chlorides) are volatilized, and the muriate of iron re-appears with its red colour.

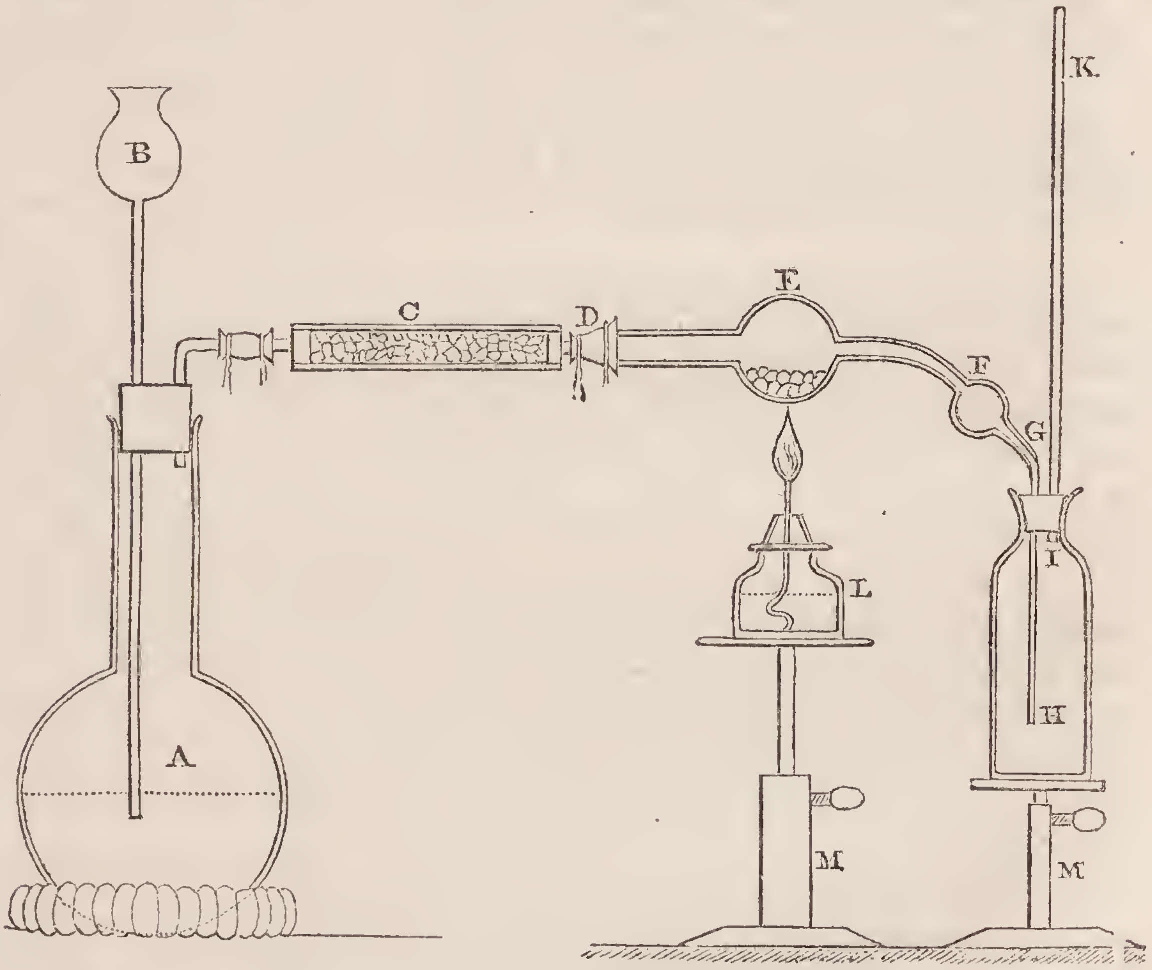
Twelve hours are sufficient for finishing the operation. The chlorine produces no partial decomposition; the whole ore combines with it, and what remains insoluble after the process has suffered no change. It is not at all necessary, therefore, to wait till all the powder be decomposed.

When we stop the operation, a part of the volatile acids (chlorides?) still adheres to the sides of the little apparatus, from the large ball to the opening of the tube H. To clear it off, we heat the two balls at once, to such a temperature only as will not volatilize the chloride of iron, and during the cooling of the balls, we introduce by the funnel B a solution of carbonate of potash, which causes a rapid disengagement of carbonic acid gas, by which the last acid vapours are swept away. When we have finally removed D E F G H, we dip G H several times in pure water, to remove the acid traces which may still adhere to it, both within and without, and we pour the water into the bottle. We then dissolve the metallic chlorides in the water. That of iron is quickly dissolved, but that of nickel resists the water at first. We must add to the water a single drop of muriatic acid, to prevent the liquor from becoming troubled. We now filter and weigh the undissolved part.

The liquid contains muriate of protoxide of iron, mixed with that of the peroxide. It is the proto-chloride of iron which forms first, and which, usually enveloped in that of nickel, is thereby hindered from combining with a new dose of chlorine. In order, therefore, to bring the iron to the peroxide state, we we must add nitric acid, and heat to ebullition. We then saturate with ammonia, precipitate the iron by the succinate of that alkali, adding finally an excess of ammonia, to be certain that no substance, insoluble in this alkali, remains in the liquid. The ammoniacal liquid must be diluted with a great deal of water, as free as possible from atmospheric air; and the oxide of nickel is to be precipitated by caustic potash. The oxides of cobalt and copper, remaining in the solution, are deposited during the evaporation of the ammonia. The silica is to be sought for in the alkaline liquor, by saturating this with muriatic acid, and evaporating to dryness. Water re-dissolves the salts, but leaves the silica. The oxide of nickel, as well as that of cobalt, contains silica sometimes, which must be separated by the solution of the oxide in muriatic acid, and by eva-

poration to dryness, which renders the silica insoluble. If any copper be associated with the nickel, in the potash precipitate, it may be separated from an acid solution by sulphuretted hydrogen, which does not act on the latter metal.

The water in which the acid vapours have been condensed, contains arsenic and sulphur. But if the mineral contained, at the same time, bismuth, zinc, antimony, or tin, the muriates of these metals would be also found in the liquid. This last circumstance would render the analysis extremely complicated. M. Berzelius, for this reason, passes over it in silence. The bottle H I must be furnished with a ground stopper. The interior of the tube I K being washed, we cork the bottle, and leave it in a warm place, till the greater part of the precipitated sulphur be acidified. If there remains any part of it, we open the bottle, and make the liquid boil; the sulphur is agglutinated, and may then be conveniently washed, dried, and weighed.



To be certain that the acid liquor contains neither iron, carried along by an ill-conducted operation, or other metals, whose chlorides are volatile, we saturate it as exactly as possible with caustic potash. If a precipitate fall, we collect and examine it; then we render the liquid acidulous again, and precipitate the sulphuric acid by muriate of barytes. We next pour into the residuary liquid a solution of a known quantity of metallic iron in nitric acid, and precipitate the oxide of iron and arsenic acid by ammonia in excess. If this operation

be postponed till the other ingredients have been determined, we may calculate with more precision the quantity of iron necessary to precipitate the arsenic acid. For 1 atom of arsenic we take 2 atoms of iron, which makes, according to Berzelius, the relative weight of 3 parts of iron to 2 parts of arsenic. Though an excess of oxide of iron augments the bulk of the precipitate, it contributes, however, to render the subarseniate less gelatinous, and more easy to edulcorate. We ought to heat the subarseniate at the fire two successive times, to be sure that it can lose nothing more, for a small quantity of sulphuric acid often adheres to this precipitate with much energy. As arseniate of barytes is also an insoluble compound, Berzelius endeavoured to separate the arsenic acid from a neutral solution by muriate of barytes, but he found anomalous results, that render this method of no value *.

M. Peschier of Geneva, has published lately elaborate analyses of different varieties of mica, the results of which are as follows:

	Silica	Alum.	Mag.	Lime	Iron	Pr. Titan	Mang.	Soda	Lith.	Pot.
1. Green mica of Vesuvius	45.7	31.7	0.95	10.75	6.8	0.1	a trace			
2. Black mica of Vesuvius	42.0	8.35	0.	15.7	8.35	15.0	......	8.5	2.5	
3. Fol. black mica of Sib.	35.5	11.25	—	—	16.0	30.0	a trace	1.7	—	6.1

The latter specimen came from the Uralian mountains, as well as the mica analyzed by Klaproth, who detected no titanium in it. His analysis gave, silica, 42.5; alumina, 11.5; magnesia, 9; oxide of iron, 22; manganese, 2; potash, 10; and loss, 1. M. Peschier says he has also discovered titanium in the white mica of Siberia, with short rounded plates and metallic lustre, as well as in the mica of Massachusetts. This is the first time that titanium has been recognised in the micas; Mr. Peschier is prosecuting his researches to see if it be one of their general constituents. It was at the invitation of M. Soret, whose optical researches on mica have been so much admired, that M. Peschier undertook their chemical investigation. Each of the above specimens has only one axis of double refraction †.

VII. *Applications of Chemistry to Medicine and the Arts.*—We perceive nothing of consequence in the foreign journals on the last subject. What belongs to medicine has been already discussed under vegetable chemistry. The quantity of original matter in the present Number of the Journal, precludes the account of optics, promised in our last.

VIII. Among the many curious papers on *Electro-magnetism*, with which Gilbert's Annalen have been lately enriched, we have been particularly pleased with that of M. J. J. Prechtl, of

* *Annales de Chim. et de Phys.* xvii. p. 113.
† *Jour. de Phys.* xciii. p. 241.

Vienna, inserted in the third number of the seventeenth volume. He coils round a glass tube or wooden cylinder, steel-wire covering the surface as with a continuous sheath. To one end of this cylindric spiral, he applies the south or north pole of a magnet, and draws it along the cylinder in a straight line, parallel to the axis. In this way a magnet is formed, which possesses the following properties :

(*a*) Along its whole length, it has on one side the north, and on the opposite side the south pole.

(*b*) These transversal magnetisms are in every point of the length of the wire-cylinder, equally strong.

(*c*) Both of its ends exhibit on the contrary no particular polarity, and they have no other magnetism than that which belongs to every individual point of the whole length. Thus the transversal magnet is in the same condition as the conjunctive wire of the voltaic column.

(*d*) If we hold this transversal magnet over a magnetic needle, in the declination-plane, it repels exactly like the conjunctive wire, the north pole of the needle to the right or to the left, according as the effective north-pole of its transversal magnetism lies to the left or to the right hand; and with greater or less force, according to the strength of its magnetism, even to 90 degrees.

(*e*) If we draw the one pole of a magnet along this transversal-magnet, in a spiral direction, the wire becomes magnetized longitudinally; the transversal magnetism disappears, the two poles are found at the two extremities, and it now resembles an ordinary magnetized steel wire. The longitudinal and transversal magnetisms are not compatible with each other, in their full exhibition.

Besides this transversal magnetism with single polarity, magnets may be easily made, possessing several transversal imagnetisms. Take four magnetic bars, about a quarter of an inch thick; provide a small disk of wood, with an aperture of about an inch diameter in its centre, and four grooves, cut in one face of the disk, leading from the circumference, inwards. in these grooves the magnetic bars are to be fixed, with their narrow edge outwards, and their ends projecting into the aperture. Let a north pole alternate with a south pole in the circle, so that the ends of the bars right opposite to each other may be homologous; and adjust the whole, so that they may touch the circumference of a steel wire-coil, about half an inch diameter. We then draw this spiral wire, through the opening and between the four magnets, taking care that the cylindric coil does not revolve on its axis, but that the direction of each individual magnetic pole remains in the same plane with the axis; for, otherwise the wire would acquire the longitudinal magnetism. Instead of the wire-coil, we may take a massive cylinder of steel. This will acquire, when treated in

the same way, the compound transversal magnetism, without its extremities having a stronger magnetism than each of its cross sections. These transversal-magnets with manifold polarities, have all the above enumerated properties of the single transversal magnets, but their phenomena are still more in unison with those of the electrical conjunctive wire; for, *under* this manifold polarized magnet, the deviation of the needle is the reverse of that *above* the magnet.

In the following manner we may produce a section of a manifold polarized transversal magnet in a larger rod, and with a proportionally greater number of poles. We take a ring of steel wire, from four to six inches diameter, and furnish it with as many poles as we can apply to its circumference. Lay the ring flat on a table, and apply to its outer edge the two poles of a horse-shoe magnet, that are as near each other as possible; then remove the magnet and apply it in succession all around, at distances equal to the width of its own poles. In this way it is easy to induce on a steel ring, of about five or six inches diameter, from twenty to thirty opposite poles.

This steel ring which represents a section of the manifold polarized transversal magnet, exhibits relatively to the greater extension and smaller number of the existing magnetisms, the phenomena which a section of the conjunctive-wire presents. In every part of it the magnetic needle with its north end above it, stands to the right hand; below it, to the left; and inversely. M. Prechtl thinks that these direct results leave no further doubt concerning the magnetical condition of the conjunctive wire.

IX. Natural History.—It results from the researches of Baron Humboldt, that in all the temperate zone, the glumaceous and composite plants, form together more than a fourth of the phanerogamous, (plants with visible flowers, to distinguish them from the cryptogamous.) The forms of organized beings are found in mutual dependence. The unity of nature is such that these forms have limited each other, according to constant and immutable laws. When we know for a certain point of the globe the number of species which a great family, (for example, that of the glumaceous, composite, or leguminous,) presents, we may estimate, with much probability both the total number of the phanerogamous plants, and the number of species which compose the other vegetable families. Thus by knowing, under the temperate zone, the number of the cyperaceæ or composite, we may guess that of the gramineous or leguminous. These estimates shew us also in what tribes of vegetables, the floras of a country are still incomplete. The geography of plants may be considered as a part of terrestrial physics.

On comparing the different systems of grouping in the two

worlds, we find in general in the new, under the equatoreal zone, fewer *cyperaceæ* and *rubiaceæ*, and more of the *compositæ*; under the temperate zone, fewer *joncaceæ*, *labiatæ*; *umbelliferæ*, and *cruciform*, and more *compositæ*, *ericineæ*, and *amentaceæ*, than in the corresponding zones of the ancient world. The families which augment from the equator towards the pole, (according to the *method of fractions*,) are the *glumaceæ*, the *ericineæ*, and the *amentaceæ*; the families which diminish from the pole towards the equator are, the *leguminous*, the *rubiaceæ*, the *euphorbiaceæ*, and the *malvaceæ*; the families which seem to attain the *maximum* under the temperate zone, are the *compositæ*, the *labiated*, the *umbelliferæ*, and the *cruciform*.

To the tabular view of quotients or fractions, which indicates the relations of every family to the entire mass of the phanerogamous plants, might be added a table, in which would be compared together the absolute numbers of the species. The following is a fragment which embraces only the temperate and the frozen zone;

	France.	North America.	Lapland.
Glumaceæ	460	365	124
Compositæ	490	454	38
Leguminous	230	148	14
Cruciform	190	46	22
Umbelliferæ	170	50	9
Caryophyllatæ	165	40	29
Labiatæ	149	78	7
Rhinantheæ	147	79	17
Amentaceæ	69	113	23

On examining in detail what we already know on the ratio of the monocotyledonous to the dicotyledonous, we observe that the denominator becomes progressively smaller, (and with the greatest regularity,) in going from the equator towards the 62° of north latitude; it augments, perhaps, anew in regions still more northerly on the coast of Greenland, where the gramineæ appear very rare. The ratio varies from 1-5th to 1-8th in the different parts of the tropics. According to Mr. Brown, this ratio is every where 1-5th in the ancient continent, (in India, in equinoctial Africa, and in New Holland). Under the temperate zone we find that the monocotyledonous are to the bicotyledonous,

In Barbary	as 1 to 4.8
Egypt	„ 1 „ 5.0
Caucasus and the Crimea	„ 1 „ 6.0
Kingdom of Naples	„ 1 „ 4.7
State of Venice	„ 1 „ 4.0
France	„ 1 „ 4.7
Germany	„ 1 „ 4.0
Switzerland	„ 1 „ 4.3

British Isles	.	as 1 to 3.6
North America	.	,, 1 ., 4.6
Under the frozen zone, in Lapland		,, 1 ,, 2.8
,, ,, Iceland		,, 1 ,, 2.8

As the monocotyledonous love humidity, they are more numerous on the British islands, and rarer in Egypt and on the arid mountains of Caucasus.

In the most fertile portion of Europe in the centre of the temperate zone an extent of country of 30,000 square leagues nourishes nearly 6,000 species of plants, of which 2,200 are acotyledonous or cryptogamous, and 3,800 phanerogamous. Among the latter there is nearly 500 *compositæ*, 300 *gramineæ*, (exclusive of the *cyperoideæ* and *joncaceæ*,) 250 *leguminous*, and 200 *cruciform*, but only 70 *amentaceæ*, 50 *euphorbiaceæ*, and 25 *malvaceæ*. The great families form from $\frac{1}{7}$ to $\frac{1}{20}$, the small under $\frac{1}{50}$ of the total mass of the phanerogamous; this is the mean state of vegetation in Europe in fertile districts between 42° and 50° of N. latitude. We may consider the ciphers indicated in the following table as the *co-efficients* of each family, for by multiplying the number of phanerogamous plants of the temperate zone of Europe by 0.75, or 0.53, we find the number of species which compose the families of the gramineæ, or the cruciform.

	France	Germany	Temp. America	Lapland
Compositæ .	$\frac{1}{74}$ or 0.135	$\frac{1}{8}$ or 0.125	$\frac{1}{6}$	... $\swarrow$
Glumaceæ .	$\frac{1}{79}$,, 0.127	$\frac{1}{71}$,, 0.141	$\frac{1}{8}$	... $\nearrow$
Gramineæ alone	$\frac{1}{13}$,, 0.077	$\frac{1}{13}$,, 0.077	$\frac{1}{10}$	... $\swarrow$
Leguminous .	$\frac{1}{16}$,, 0.063	$\frac{1}{18}$,, 0.056	$\frac{1}{19}$	... $\swarrow$
Cruciform .	$\frac{1}{19}$,, 0.052	$\frac{1}{18}$,, 0.056	$\frac{1}{62}$	... $\swarrow$
Umbelliferæ .	$\frac{1}{21}$,, 0.048	$\frac{1}{22}$,, 0.046	$\frac{1}{57}$	$\frac{1}{55}$ $\swarrow$
Labiatæ .	$\frac{1}{24}$,, 0.042	$\frac{1}{26}$,, 0.038	$\frac{1}{40}$	$\frac{1}{70}$ $\swarrow$
Cyperaceæ alone	$\frac{1}{27}$,, 0.037	$\frac{1}{18}$,, 0.056	$\frac{1}{40}$	... $\nearrow$
Amentaceæ .	$\frac{1}{50}$,, 0.020	$\frac{1}{40}$,, 0.025	$\frac{1}{25}$	$\frac{1}{21}$ $\nearrow$
Orchideæ .	$\frac{1}{67}$,, 0.015	$\frac{1}{43}$,, 0.023	..	... $\swarrow$
Boragineæ .	$\frac{1}{74}$,, 0.014	$\frac{1}{72}$,, 0.014	..	... $\nearrow$
Rubiaceæ .	$\frac{1}{73}$,, 0.014	$\frac{1}{70}$,, 0.014	..	... $\swarrow$
Euphorbiaceæ .	$\frac{1}{70}$,, 0.014	$\frac{1}{100}$,, 0.010	..	... $\swarrow$
Joncaceæ .	$\frac{1}{85}$,, 0.012	$\frac{1}{94}$,, 0.011	$\frac{1}{152}$	... $\nearrow$
Ericineæ .	$\frac{1}{125}$,, 0.008	$\frac{1}{90}$,, 0.011	..	... $\nearrow$
Malvaceæ .	$\frac{1}{140}$,, 0.007	$\frac{1}{230}$,, 0.004	$\frac{1}{125}$	... $\swarrow$
Coniferæ .	$\frac{1}{192}$,, 0.005	$\frac{1}{269}$,, 0.004	$\frac{1}{103}$	$\frac{1}{160}$ $\nearrow$
Ericineæ & rosages	$\frac{1}{125}$,, ...	... ,, ...	$\frac{1}{36}$	$\frac{1}{25}$

Explanation of the Signs.—$\nearrow$ the denominator of the fraction diminishes from the equator, towards the north pole; $\swarrow$ the denominator diminishes from the north pole towards the equator

X. Economics, *or Applications of Science to the ordinary purposes of life. Preservation of Grain.*—In the present posture of agricultural affairs, any plan which could promote the preservation of grain merits public attention. In the year 1819, M. Clement-Desormes published in the *Journal de Physique* an ingenious paper on this subject, of which no account, we believe, has been given in this country.

It is well known, that water is the grand instrument of the decay and destruction of all organized matter. Vegetable and animal substances, deprived of moisture, will keep for any length of time, if they be protected from the depredations of insects. But these insects cannot themselves exist, or soon perish, in an air hygrometrically dry. M. Clement ascertained this important fact, on the insects which habitually prey on grain. He therefore proposes to construct granaries of cast-iron, in the form of great cylinders or parallelopipeds, which shall be entirely filled with grain, and into which no air shall be suffered to enter, till it has passed through a body of unslaked quicklime. But as the air, interspersed among the grain, will vary in volume, with the variations of the barometer and thermometer, he attaches a valve to one extremity of the great iron case, which allows the expanding air freely to escape, while the equilibrium of pressure will be restored, when the bulk of the included air contracts, by the passage of the dry air over the lime, through a valve opening inwards. Since it is of consequence to be able to inspect any part of the body of grain at pleasure, he provides the iron magazine with two or more vertical wells, consisting of square or round pipes of cast iron, open within, but closely joined to the great iron case at top and bottom, and sliding valves placed at different points of this well, may be opened by appropriate rods, and will thus exhibit the state of the included grain without laying open the main magazine to the moist air of this climate. A magazine of this kind may be built, at the present price of cast iron, much cheaper than one of equal capacity of storage in stone or brick-work; for if it be 20 feet deep, it will serve for as much grain as a building of 10 stories high, and of the same horizontal area; allowing, in common granaries, the grain to lie two feet deep. But a leading advantage of M. Clement's plan, is the saving of the manual labour, required in the common way, for turning over the grain from time to time. As the grain is secluded from moist air, and all equally dry in the iron magazine, there is no necessity whatever for that troublesome and expensive operation.

The expense, we believe, will be much less to build an iron than a common granary; so much so, that we have no doubt farmers will eventually preserve their grain in a similar way. Supposing the house to be formed of large iron plates,

with flanges for bolting them together, with packing of lead, as in water pipes, it will be most economically and conveniently made in the quadrangular form. Across the bottom of one end, an iron pipe, having a number of small holes, or thin slits, might be laid, from the middle part of which a similar pipe would rise to the top. This might be the receptacle of the lime, reduced to pieces about the size of a plum. A valve at top, opening freely inwards, would admit the external air, when the equilibrium required it. At the other end of the chamber, bu near to the middle height, might be placed another horizontal iron pipe, also perforated with little holes, from the middle of which would ascend a vertical tube, considerably above the top of the magazine. Here would be fitted a valve, opening outwards. If the grain had been stored in a somewhat damp state, then a current of desiccated air could be easily determined through it, so as to render it speedily dry, by establishing a small fire in the *eduction* pipe above the top of the chamber. Thus a constant current of air would pass down through the lime, and, in its hygrometrically dry state would sweep through the body of grain, necessarily robbing it of its pernicious moisture. We need not enter into further particulars concerning the number and forms of these pipes, &c., which will suggest themselves to every man of sound scientific principles. In such a magazine, we believe that well-cured grain, might be preserved for any length of time without deterioration; and at no further expense than the interest of money on the first cost of the building. A pent-house roof should be stretched over the chamber, to screen the workmen from rain when they are introducing or discharging grain, inspecting its condition, or changing the lime, which may be easily done by having a false bottom to its cylinder, capable of being drawn up by an iron rod fixed in its centre. The openings in the top should be shut, after Glauber's plan, with grease-lute.

Art. XVI. ANALYSIS OF SCIENTIFIC BOOKS.

i. *Voyage en Ecosse, et aux Isles Hébrides, par* L. A. Necker de Saussure, *Professeur Honoraire de Minéralogie et de Géologie à l'Académie de Génève, Membre de la Société de Physique et d'Histoire Naturelle de Génève, Membre Honoraire de la Société Géologique de Londres, et de la Société Wernérienne d'Edinburg, &c.* 3 *tomes,* 8*vo. pp.* 1549. *Génève et Paris.*

This is a very pleasing work; the production of an amiable and accomplished mind. The author's principal object is geology; a subject which his studies in the great school of

the Alps, render him well qualified to discuss; while strict attachment to truth, in contempt of the allurements of hypothesis or system, gives a rare valúe to his descriptions. This quality will be most gratefully acknowledged by all the dispassionate readers of the polemical geognosy of Scotland, in which identical phenomena are very frequently painted in totally different colours and forms, by the contending votaries of Neptune and Pluto. Whether the actual position and distribution of the inert mineral masses which compose the crust of our globe, have resulted from the agency of water or fire, must appear to every man of common sense, a question in science, susceptible of very tranquil investigation, one little calculated to kindle dissension or awaken animosity. It is, however, too true, that the northern capital of this island continued for many years a scene of such strife among its *geologists* and *geognosts*, as their subject itself could alone have rivalled, had Vesuvius burst forth in the middle of their Firth.

This world-making mania which split Edinburgh into factions, is itself not the least geological curiosity in Scotland, and deserves inquiry into its causes, as well as the juxta-position of barren rocks. Contemplated simply with a philosophical eye, without any sarcastic reference towards the *intellectual city*, the above phenomenon might perhaps be traced to some or all of the following causes: 1st. To the practice at the Royal Society of Edinburgh, of entering into a verbal discussion, often somewhat animated, on the papers which are read before them. 2d. To the predominance of metaphysical disquisitions in the Scotch Universities, from which a habit of indefinite argumentation is often generated; the reverse frame of mind to that required in physical investigation. A person of this temper, will dispute for victory with unwearied vehemence. To this cause we must ascribe the extraordinary number of debating societies in Edinburgh. 3d. To the great influence which the men of law, whose task it too often is to refine on trifles, and "make the worse appear the better reason," exercise on Edinburgh Society; an influence due to their acknowledged talents, as well as numbers. These concurring circumstances give, perhaps, an undue predominance to the syllogistic logic which costs nothing, over the sounder but less shewy logic, of Bacon. It was, perhaps, this scholastic spirit which unhappily operating on the powerful and elegant mind of the late Professor Playfair, led it, though admirably calculated for inductive research, to expend itself less profitably on the hypotheses of Dr. Hutton. Having thus enumerated what appear to be the main causes of the late geological frenzy of the Scottish capital, we shall now proceed on our tour with the Genevese philosopher.

Though mineralogy was the chief end of Mr. Necker's journey through Scotland, he judiciously extended his views

to many other objects interesting to every enlightened traveller; and has thus composed a far more readable book than geological tours have usually been, always excepting the *Voyages dans les Alpes* of M. de Saussure. "The study of manners," says he, "considered in a general point of view, and in reference to the ancient constitution and peculiar condition of Scotland, were the subjects which next to natural history, captivated my attention, during my residence in that interesting country. The length of my visit; the inappreciable happiness which I enjoyed of living in the society of celebrated men, who honoured me with their friendship, or aided me with their advice; and lastly, my acquaintance with the different dialects spoken in the British Isles, put me in the most favourable position for observing with advantage."

We can, indeed, easily imagine, that an ingenious young man of agreeable manners, coming to prosecute his studies in Edinburgh, from that illustrious republic which gave to Scotland its revered system of ecclesiastical polity and faith, would be hospitably received; but when we recollect that this stranger was the grandson of the minister Necker, and of the philosopher de Saussure, as also the kinsman of Madame de Staël, we are sure that every facility would be afforded to his researches. We are happy to find that such opportunities have been well bestowed; and, after the interval of a few years, yield a rich return of generous feeling and information. Some English readers may probably think that Mr. Necker has occasionally been led through warmth of heart, to eulogize the northern part of the British island, somewhat at the expense of the southern. It appears that on his arrival in Scotland, he was under the influence of certain prejudices against the natives of that country, "which were in vogue among the English;" and, he accordingly takes some pains to shew, that the reproaches bestowed by the latter people on their brethren of the North, are altogether unmerited. His intention is no doubt laudable; but we believe that the sarcasms circulated in England against the sister nation by Wilkes, Johnson, and the author of Junius, have now little currency, and no influence whatever in obstructing the industry of the Scots. This pushing people have contrived in various ways to secure their full share at least of all the loaves and fishes in the civil and military appointments, both at home and in the colonies.

The Scotch geologist has little need to wander far in quest of gratification, however strong his migratory tendencies be in other pursuits. The very nakedness of the country, the total absence in many districts of soil and vegetation, present to the geological student its chief value and attraction. Thus Mr. Boué in his *Essai Géologique sur l'Ecosse,* lately published, says, "I saw *with pleasure* the nearly naked mountains of that country, and

the rugged precipices on its shores, which permitted me to trace at leisure, during a journey of many days, not merely the relations of a few strata, but several geological formations, sometimes under very singular circumstances."

The untrammelled spirit of observation which Mr. Necker uniformly breathes amid the seductions of system and the tyranny of names, merits the highest commendation. *Nullius in verba* is obviously his travelling talisman, as it ought to be that of every philosopher. Full credit may therefore be given him for the following declaration in his Preface: "In all these researches it was my duty to detach most perfectly the impartial relation of facts from every thing hypothetical and systematic. The traveller who recounts his observations ought, in my opinion, to be regarded as a witness who depones to the verity of a fact, without being obliged to emit his conjectures as to its causes. In this spirit I have followed, although imperfectly without doubt, the path so skilfully traced out to geologists by my illustrious grandfather, De Saussure, a path truly philosophical, of which his *Voyages dans les Alpes* exhibit an admirable outline. Leaving to others to decide on the origin and mode of formation of mineral masses, I have had in view only to examine with attention the phenomena which nature presents to us; and if I have occasionally sought to draw some inferences from this examination, I have never ventured beyond the more immediate consequences, those which geognosy authorizes, such as, without assigning the agent which may have presided at the formation or consolidation of rocks, fix the relative antiquity of the different masses, agreeably to principles universally adopted at the present day."

It was during the years 1806, 1807, and 1808, that Mr. Necker was in Scotland. Had the work, now first published, related to politics, or to the arts and sciences in general, the delay would have greatly diminished its value. But in reference to its geological purport, the interval has been well employed, and has brought to the original stock of observation a great accession of interest. Since his departure from Scotland he has surveyed several districts of the Alps of Switzerland and Savoy; he has visited a part of Auvergne, of the Vivarais, the Pyrenees, the coasts of Provence, and of the Genoese states, and journeyed, for the sake of geology, through many counties of England, particularly Cornwall. Hence he has been enabled to compare the geological structure of these different regions with that of Scotland and its neighbouring isles. And he justly remarks that, whether it be as a painter or naturalist that we observe different countries, it is *comparison* only which can teach us to seize in each its characteristic features.

"The geological description," says he, "which Professor

Jameson of Edinburgh has given of certain districts of Scotland, and of some of the Hebridian isles, having never been translated into French, is hardly known in the countries where this language is spoken. Even did they know it, I may venture to assert, without pretending to take from the merit of the many valuable observations with which the works of this learned mineralogist are replete, that a too scrupulous attachment to the doctrine of his illustrious master, Werner, has injured the accuracy of some of his observations. He has even neglected sometimes to state facts which, by their consequences in reference to geological methods, deserved to be examined with the greatest care; facts which characterize the mineralogical region of Scotland, but which have been somewhat too lightly omitted by the learned disciple of the Freyberg geologist, because they exhibited a manifest opposition to certain points of the system of Werner. It was to the study of these phenomena that I most particularly devoted myself; and without wishing to deduce theoretical conclusions, I have sought to verify their existence, and ascertain the consequences which result from them*."

Nothing can be more just than his remarks on the *legerdemain* which the fanatical Wernerians played off against the followers of the Huttonian heresy. "The former availed themselves of the ascendancy which a more minute study of minerals afforded, to depreciate the observations of their adversaries. They denied the existence of *facts* which the latter had discovered, or they tried to sink their importance. Hence it happened that phenomena, important to the natural history of the earth, have never been made known and appreciated as they ought to have been, by geologists most capable of estimating their consequences. The mixture of hypothesis with observation, and the vagueness of nomenclature in the objects described, were little fitted to command the attention of the naturalists of our days, of those philosophers who, having adopted in the study of nature a more sober march than their predecessors, seek for precise observations rather than brilliant theories, and require an exact definition of the objects that engage them. Directed by a similar conviction, I sought to verify, on the contested spots, the observations of the two schools; and without embracing the volcanic doctrine of Hutton, or the Neptunian system of Werner, both of which, in the still limited state of our geological knowledge, appear to me equally incompetent to explain the formation or the actual state of the globe, I am satisfied that the greater part of the facts on which Werner has founded his system of geognosy are truths, general, important, and which have been verified by observations repeated in dif-

* Vol. 1. Introduction, p. iv.

ferent regions of the earth. But I have at the same time found that there exist facts, neither less general nor less important, which Werner did not know; facts which his disciples have denied, and which are hitherto not duly appreciated by the greater number of geologists, who have not personally observed them; and that these facts, noticed for the first time by Dr. Hutton, have been confirmed by the subsequent observations of Mr. Playfair, Sir James Hall, Lord Webb Seymour, and others."

Mr. Necker's work is divided into four parts. The first is occupied with Edinburgh, its society, climate, and the mineralogical structure of its environs, extending to both sides of the Forth, of which there is a useful map at the end of the first volume. His second part is devoted to that most interesting of all known geological scenes, the island of Arran, which he has also illustrated with a map. Under this division we have an account of the adjoining island of Bute, and a disquisition on the manners of the Lowland Scots. His voyage to the Hebrides, which was both extensive and enterprising, occupies the third part; and in the fourth we have a lively account of the manners of the Highlanders, before and since the rebellion of 1745; disquisitions on the Gaëlic language, of which Mr. Necker has been a diligent student; on the authenticity of the Poems of Ossian, and on Gaëlic poetry and music. In an Appendix the author presents us with an interesting and well-drawn outline of the natural history of Scotland.

Before entering on a detailed description of the geological phenomena observed in the hills round about Edinburgh, and on the banks of the Firth of Forth, he gives a sketch of the general structure of the territory. This mineralogical district comprehends the whole hydrographical basin of the Forth, and a part of that of Tay. Bounded on the north by the first chain of the Grampian mountains which stretch from south-west to north-east, and on the south by the long chain of the hills of Lammermuir, this vast basin terminates to the east in the German Ocean, and to the west and south-west it graduates into the basin of the river Clyde. The bottom of this great valley, which extends from the primitive mountains of the north of Scotland, to the transition hills of the south, belongs to the class of secondary formations. The nature of the rocks which compose this territory, the peculiar position of their strata, and the great abundance of coal which they contain, have induced geologists to rank it under the independent coal formation of Werner. Here we have alternating beds of conglomerate or coarse puddingstone, of sandstone, and slate-clay, containing vegetable impressions. The presence of coal, and especially of schistous coal; the disposition of the beds in concave surfaces, or in the shape of a basin, so that the extremities of the strata rise towards the mountains on whose feet they re-

pose, while they are almost horizontal in the middle of the basin; finally, the structure of this formation, as Werner expresses it, shews that it is independent of the ordinary series of formations which compose a mountain-chain. This coal formation is parcelled out in tattered fragments, which occupy the lowest grounds of the country, without any mutual connexion among its parts. Thus, while in the above district we see it fill up an immense valley, between primitive and transition mountains, there exists, towards the Murray Firth, one of these coal districts set down, as it were, in the midst of the primitive chain; and to the south we find another in the transition mountains of the north of England. Following the sea-shore northward from Berwick, we meet at Siccar Point, the southern limit of the coal formation of Scotland. The sea has here laid open the precise point where the rocks of this formation join with the transition grauwackes of Lammermuir. Whatever is to the north of the base of these hills belongs to the coal formation, which extends over all the low districts of the counties of Haddington, Edinburgh, Stirling, Kinross, and Fife, and which is united in the county of Lanark to the coal formation of the counties of Dumbarton, Renfrew, and Ayr, which fills the basin of the Clyde. An approximate idea of the northern limit of this formation may be had, by drawing a line in the direction from north-east to south-west, from Stonehaven, a small sea-port in Kincardineshire, where Mr. Playfair placed the junction of the primitive and secondary rocks, to Callender, in Stirlingshire, where, by Mr. Necker's observations, we pass from one of these formations to the other. The coal formation of Edinburgh is separated from the grauwacke of Lammermuir by a narrow stripe of red sandstone, to which formation probably belong the sandstone of Hawthornden, near Roslin and that of Craigmillar Castle, as well as the conglomerate of the Siccar Point. This same sandstone formation is found on the northern limit of the coal country, where it reposes also on the transition rocks; for in the first chain of the Grampians we observe likewise grauwacke and a rock of quartz, feldspar, and chlorite, which appears to belong to the transition class; but this stripe is inconsiderable, and is sometimes altogether absent.

A striking feature is still wanting to this geological portrait. Above the coal region, which has been described as an immense basin, whose surface, little elevated above the level of the sea, is relieved merely with rounded hillocks, there rises at uncertain distances conical hills, or ridges of naked and precipitous rocks, of a nature totally different from the ground on which they repose. The rocks which form these hills, whose height varies from three hundred to seven hundred and sixty feet, are basalts, wackes, grunstein, and other rocks of the same

nature, as well as porphyritic slates, porphyries, and amygdaloids with a basis of wacke or clay. The above rocks are not found in beds, but in irregular masses, or arranged in prismatic columns, and sometimes in spheres with concentric laminæ. These rocks constitute the principal and characteristic mass of the formation which Werner has named *floëtz trap,* a formation which he regards as the most recent of the general and universal deposits. He considers it, with the exception of the alluvial masses, as having once entirely covered all the other rocks; and, but for the numerous revolutions and subversions which our globe has experienced, the formation of floëtz trap, or of stratiform trap, would still extend, according to this learned geologist, over the whole surface of the earth, and would thus conceal the more ancient formations.

Mr. Necker considers this hypothesis as inadmissible, because it assigns to the trap formation an extent by far too great. In fact, it occupies at present but a very small surface comparatively with the general formations, such as those of the granites, micaceous and argillaceous schists, and especially the sandstones and limestones so abundantly spread over all the parts of the earth with which we are acquainted. The floëtz trap formation, on the contrary, dispersed in insulated parcels, and at great distances from one another, occupies merely certain districts, which have no mutual relation. Large countries are entirely without them. They are not found in the great chains of the Alps, of the Pyrenees, and of the mountains of Sweden and Norway, which comprise, however, a complete succession of the general formations. Other reasons still more powerful oppose the opinion of Werner, relative to his stratiform traps, and authorize us to regard their formation as being of a nature very different from that of the other classes of rocks, and to assign them, in the system of geognosy, a place altogether distinct.

It has been frequently remarked that from the most ancient primitive deposits whose appearance is entirely granular and crystalline, to the most recent formations which have a dull and earthy aspect, the rocks seem to have diminished in purity, hardness, and tenacity, by a constant gradation. Yet here we have the formation of floëtz trap, which seems to interrupt this uniform march of nature, by presenting to our view the same appearances, the same structure as the primitive rocks, although its origin be comparatively very modern. It has also been often observed, that the more recent the formation of rocks, the greater number the variety of animals or plants they contained under the form of petrifactions or impressions. If then, as Werner supposes, the floëtz trap be one of the most modern of the great universal deposits formed by precipitation, we ought, by analogy, to suppose it replete with animal or vegetable spoils

in much greater abundance than the stratiform formations on which it reposes. But the very reverse of this is the case; the majority of observers agreeing in the fact, that basalts, and the rocks of the same nature, contain no trace of organized matter. Kirwan, indeed, and some German mineralogists, have asserted the presence of shells in basalt and in wacke; but these shells, in number inconsiderable, and of undetermined genera, have been observed only in a very few localities. And even should it be proved, which it has not, that the rocks which contain them are true basalts and true wackes, it would be no less certain that fossil shells are the greatest rarities in this class of rocks. Mr. Necker affirms that in those which he has himself observed in Scotland, in Auvergne, in the Vivarais, and in Languedoc, he has never perceived the slightest trace of organized bodies. The preceding facts lead him to consider the basaltic rocks as a product of different origin from the others, or a formation *sui generis*, and he calls it accordingly the *independent trap formation.* In retaining for this class of rocks the generic title of traps, which several German and French mineralogists have given it, he desires it to be understood that the traps of the Swedes, and of M. Faujas, do not belong to this division. In fact, these latter are transition rocks, *cornéennes*, varieties of petrosilex, or siliceous schists, often even greywackes, of a very fine grain and compact texture. He also excludes the rocks of hornblende of the primitive formation, as well as the transition grunstein, although Werner has grouped all these rocks under the denomination of trap. In reality, a comparative examination of the grunstein of transition, with the trap grunstein, discloses marked differences in their composition and in their structure. In the last, the hornblende exists in greater abundance; it is presented under the form of short and narrow plates, while, in the first and in the primitive amphibolic rocks, it affects that of elongated parallelopipeds, or needles.

The characters of composition and position, which distinguish the trap rocks are, the cohesive aggregation of their constituent parts, their more or less granular texture, the almost total absence of petrefactions. The structure has this peculiarity, that these rocks are not divided into parallel beds, which preserve through a long space the same direction and the same inclination. They occur under the form of prismatic columns, of balls composed of concentric spherical laminæ, or of tables, but most frequently in shapeless masses, and at other times in beds, or in veins, which penetrate the rocks of every class of formations, from granites to sandstones and slate clays. The rocks of the trap formation repose on every other, occasionally even on those of alluvion, and are thence the newest of all. They do not form considerable mountains, nor long chains of

hills, but they are disposed in scattered fragments, and in solitary conical hills, which rise here and there out of the bosom of the plains. Sometimes, on the top of a mountain composed of rocks of a different formation, there starts up a ridge of trap rocks, vertically like an enormous wall. Lastly, the character which distinguishes them from the greater part of known rocks, is their property of causing singular modifications and remarkable changes on the rocks on which they repose, or which they penetrate in the form of veins. This truth, hitherto little attended to, has been insisted on only by Dr. Hutton and his pupils.

The greater part of the hills round Edinburgh belong to the trap formation. Arthur-seat, the Calton-hill, the rock on which the castle is built, Braid and Blackford hills, and a part of the Salisbury-crags, are trap-rocks ; as well as several masses on the northern bank of the Firth of Forth, such as the rocks of North Queensferry, of Burntisland, of Kinghorn, and Kincaid near Ely. On the southern shore of the gulf we have also the hill named Taprene Law, above Haddington ; the rock of the Bass ; North Berwick Law ; the islands of Faidra ; Craigleith, and the rocks of Dunbar. The greater part of the Pentland-hills, also, probably belongs to the same formation.

The great precipitous rock on which the castle of Edinburgh is built, consists entirely of a beautiful black basalt, whose surface exhibits, from decomposition, a whitish or yellowish crust. Here we have neither beds nor prismatic columns, but a mass divided by numerous fissures into irregularly angular pieces. On a comparison of specimens, Mr. Necker finds that the basalt of the columns at the base of the Puy de Dôme, in Auvergne, on the road between this mountain and Clermont Ferrand, is precisely the same as that of Edinburgh, and that the basaltic mole of Mont-Fernier, near Montpellier, differs from the above only by the argillaceous odour it exhales when breathed upon. In the western part of the Castle-hill, at the bottom of the basaltic rock, the horizontal beds of sandstone, which support it, have been discovered. This sandstone is white, singularly hard and compact, and belongs to the coal formation that forms the elevated ground on which the Old and New Towns of Edinburgh stand. A perfectly distinct line of demarkation separates at the above point, the black basalt of the trap formation from the white freestone. There is no gradual transition between these two rocks so different in their nature, origin, and antiquity.

A green porphyry, of an earthy consistence, covering a conglomerate, forms the greater part of the Calton-hill, that picturesque eminence, which every visiter remembers with delight. The base of the porphyry is a wacke of a dark-green colour, having a dull, earthy fracture, and affording, when breathed on, a strong argillaceous smell. This is the paste in which are

enclosed rhomboidal crystals of feldspar, of a deep flesh red, from two to five lines long, as also prisms of augite or basaltic hornblende. It contains, likewise, some thin plates of calcareous spar. The earthy and friable nature of this porphyry, renders it susceptible of speedy decomposition when it is exposed to the action of the weather. Hence one cannot look without regret, on the crumbling substratum which now sustains the lofty column reared on this hill to the memory of Lord Nelson. One of the principal effects of the action of the elements has been to destroy and remove entirely, even to the depth of several inches, the crystals of feldspar, and the little zeolites, contained in the argillaceous paste, leaving in their place a multitude of holes. The rock then resembles a porous lava. Some mineralogists, deceived by this appearance, and observing but superficially the porphyry of the Calton-hill, have hastily concluded that it was the remains of a stream of lava, poured out by a volcano. In the mass of the porphyry, we find abundance of arsenical pyrites, and in the veins of calcareous spar which traverse it, we meet occasionally with a variety of analcime, of a flesh-red colour, to which the name of *sarcite* has been given. This mineral is opaque, its fracture is unequal and earthy; it is commonly found crystallized in a solid of twenty-four sides, but the crystals are rarely single and perfect. They occur rather in confused groups.

To the south of the town we have two other hills of the trap formation; Salisbury-crags and Arthur's seat: the first under the aspect of a steep mountain, terminated at the summit by a long ridge of precipitous rocks, resembling the little Salève; the second loftier, and of a conical shape, recalls the figure of the Mole as seen from Geneva. Mr. Necker classes the former with the floëtz trap of Werner, in opposition to Mr. Jameson, who ranks it with the coal formation. It possesses, in fact, all the geognostic characters of the trap formation. In this hill, the scene of so many contests between the rival geologists of Edinburgh, we have three objects worthy of successive examination. 1*st*, The strata of sandstone, which compose its base and most considerable part; 2*d*, The mass of grunstein which forms its crest; 3*d*, And, lastly, the phenomena presented by the two genera of rocks at their points of mutual contact. A distinct line of demarkation separates them, and allows the circumstances which accompany this junction to be clearly observed, over a long extent. The beds of sandstone run from south-west to north-east; they dip to the south-east and rise to the north-west, forming, with the horizon, an angle of about 60°. The mass of grunstein has no appearance of stratification. It follows nearly the same direction and the same inclination as the sandstone; and the precipice is turned towards the west, as are all the rocky façades near Edinburgh.

The grunstein wears thinner towards the northern extremity of the hill, where it seems to terminate in the form of a wedge; a phenomenon which cannot be ascribed to the destructive action of the elements, since the thinnest portion is now covered by other strata which still exist in their natural position, and thus have preserved this part from degradation. A hornblende of a greenish-black, or bottle-green colour, constitutes two-thirds of the grunstein rock; the rest is a feldspar, which varies from red to white. Small transparent plates, or milk-white globules, of calcareous spar, are scattered through it.

The grunstein, by long exposure to the weather, is decomposed, loses its lustre, assumes an earthy aspect, and a colour of a livelier green. The angular and irregular masses of which the rock is composed, lose their angles, and form balls composed of very thin concentric laminæ, which are separated and destroyed in succession. The upper surface of the rock is that which has been most exposed to the action of the elements. We find the grunstein has taken the precise appearance of a porous lava.

Let us now see what happens at the junction of these two mineral masses. The principle of the superposition of rocks, on which the whole of modern geognosy rests, seems to Mr. Necker but one of the objects of this study. This principle shews to a certain point the relative antiquity of the different classes of rocks, but it goes no further. We must examine the rocks in themselves, their structure, and their relations with one another, before we can become acquainted with the agents which have co-operated at their formation, and the particular mode of this formation. Such, however distant at present, is the true end of the vast study of geology. Now, asks he, what can more promote its attainment, than to inquire if any rock has, by its presence, influenced in any manner the nature of the adjoining rocks, and introduced some modification into their arrangement or composition? It is rare to find the different species of rocks in immediate contact; it is still rarer to find such as have modified in any way by their presence, the adjoining rocks; a circumstance, perhaps, peculiar to those of the trap formation. Dr. Hutton is the first geologist who examined minutely the phenomena which accompany the junction of the trap rocks with those of the other class. The theory, however, which he invented to explain these interesting facts, appears, like all the other theories of the earth hitherto imagined, liable to insurmountable objections of both a general and particular nature.

In Salisbury-crags, nothing can be more decided, or clearer, than the line which separates the reddish-white sandstone from the dark-green diabase or whin. Hand specimens are readily procured, in which the two rocks occupy the opposite surfaces.

In proportion as the sandstone approaches the trap or whin a marked change is observed in its structure, but not in its composition; for there is no gradation, no insensible passage of the former into the latter, but the texture of the sandstone becomes more and more compact, with a corresponding increase of hardness. Some of the strata, previously straight and regularly parallel, have experienced a very remarkable inflexion, or dislocation. Thus the tender and friable sandstone, when it approaches the trap, becomes a mineral like jasper or hornstone. The red and white colours, formerly blended, now separate, and the rock exhibits a ribbony aspect. Higher up the sandstone assumes a more decided quartzose appearance; with increased density, it becomes more translucid on the edges, its fracture is scaly, and its lustre shining and vitreous. The thickness of its layers also diminishes, as the rock grows more compact; but at the very line of junction, where the quartz *seems* to predominate, we have the same strong effervescence with nitric acid, as in the ordinary sandstone, which proves the composition to be unaltered, in opposition to those who pretend that the indurated sandstone is a deposit distinct from the rest of the mountain.

The disorder and derangement of the sandstone strata near the contact, are no less remarkable; in fact, these strata, which lower down all preserve the same position and inclination in a regular and uniform manner, appear here contorted and broken in every direction. Sometimes we see beds, which having retained for some time the common direction, vanish altogether, or in part, to be replaced by masses of trap, in communication with the great incumbent rock; sometimes these beds, in bending, entirely change their direction, and form with the strata, which have retained their position, a greater or less angle. In other places we perceive large lumps, or even enormous masses of sandstone, or an argillaceous schist, completely enveloped by the whin or diabase, and thus insulated in the middle of this rock. There, however, they still preserve their strata; and these strata have the same thickness, and sometimes the same direction, as those from which they so plainly appear to have been separated, as to leave no doubt of their having once been contiguous.

The trap or whin at the place of junction, exhibits very confused crystallization; for the crystals of hornblende and feldspar are here so minute, that they seem to form together a compact homogeneous stone, which might be taken for basalt, if its deep-green colour did not ally its aspect more to that of massive hornblende. This rock effervesces with the acids, owing to the small transparent scales of calcareous spar disseminated through it. As it recedes from the sandstone, the rock insensibly assumes a more brilliant and granular fracture.

We begin to distinguish easily the shining plates of hornblende; and higher up we at length perceive the associated feldspar. The same phenomena occur in an inverse direction, in the parts of the rock, which are again covered with beds of sandstone.

The conical hill, called Arthur's-seat, is a peculiar conglomerate, named by Werner *traptuff*, though it resembles in no respect a tuff, properly so called. It consists of large blocks of sandstone, of clay iron-stone, of grunstein, and of fragments of basaltic prisms, confusedly piled up, and joined together by a paste of the nature of wacke, which is traversed by a great many veins of calcareous spar, and others of a mixture of limestone and red jasper. It is not arranged in strata, but rises into a rounded hill without regular divisions, above a succession of beds of trap and sandstone, similar to those described above. It does not conform to these rocks on which it rests. Above the conglomerate, a conical point ascends, which forms the summit, called Arthur's-seat. This point is entirely composed of very regular prisms, placed in a vertical position. The rock of which these prisms are formed is a porphyry, with a base of earthy basalt, of a dull-greyish black, difficultly scratched by steel, and easily melted before the blow-pipe into a black enamel. In this basalt are enclosed, in sufficient abundance to make them appear constituent parts, olivine, (peridot, Haüy) in very transparent small grains, of a greenish yellow, as also rectangular plates of augite, (pyroxène, Haüy) of a fine black colour, and a lively lustre; with thin plates also rectangular, of feldspar, which is easily recognised by its changing reflection of light.

Of the picturesque geological phenomena which occur on the coast between Berwick, and Siccar-point, (near Cape Keale) where the Edinburgh coal formation terminates, Mr. Necker has given a very interesting description. Here we see the sandstone in strata of little inclination, resting on the nearly-vertical beds of greywacke, which they partially cover. The direction of these two orders of strata is indeed the same, but their inclination is absolutely opposite. For while the beds of greywacke dip towards the south-southwest, and rise up towards the north-northeast, those of the sandstone dip towards the north-northeast, and rise up towards the south-southwest. There can be nothing more striking than this junction, completely laid open by the waters of the ocean. It is uncommon thus to see in immediate contact two classes of rocks of so different a formation, and one of which must have preceded the other by a long series of ages. The sandstone does not rest immediately on the greywacke, but some strata of a pudding-stone, or coarse conglomerate, having the same direction, and the same inclination as that of the sandstone, divide the two. The pudding-stone is in fact nothing but this same sandstone filled with pebbles,

some rounded and others angular, which are fragments of the greywacke which it covers. The greywacke, which composes these vertical beds, forms a part of the long belt of transition country, which traverses the whole of southern Scotland, from the eastern to the western ocean. The Lammermuir-hills consist of it, and all the cliffs on the coast, from Siccar-point to near Berwick, are the vertical section of this chain of hills of greywacke. Dr. Hutton, who was the first to describe the interesting phenomena which these shores exhibit, has improperly named primitive schist, the rock which appears in vertical strata, covered by sandstone at Siccar-point. It is neither a micaceous schist, nor an argillaceous schist (clay-slate), nor any of the primitive schists subordinate to these rocks, but it is the schistous greywacke of Werner; that is, one of the most ancient conglomerates, or mechanical deposits, which exist in nature. It is a species of very fine sandstone, entirely formed from the spoils of primitive rocks, united by a quartzose cement. The greywacke in mass, and schistous greywacke, here alternate, but the second appears to be in much smaller quantity.

In pursuing our journey towards the south, nothing is to be seen but lofty rocks of greywacke, sometimes slaty, and sometimes compact. The state of topsy-turvy disorder and contortion of these strata, is among the most striking objects in geology. The same undulations which are observed in the small *folia* of the greywacke slate of Siccar, are now represented on a grand scale. Here we find entire beds, enormous ranges, rolled together, folded up and shattered in all directions, and in all possible manners. In one place they assume the shape of an U reversed, in another of an S, and in a third of a C, the back of which is sometimes turned up, at others down. All these figures pass into one another, mingle, and succeed, so as to form the most whimsical designs, baffling all powers of description. Fast-Castle and Lumsden-Burn present these phenomena of dislocated strata in a very remarkable manner. Of these, Mr. Necker has given two good illustrative engravings, after his own drawings. A circumstance still more remarkable is, that the cause whatever it has been, which thus subverted these strata, has changed nothing in their direction, which rests invariably the same, that is, nearly from west to east; and has modified merely their inclination, which varies several times, even in the plane of a single stratum. However we may represent the effort which has changed the position of these beds of greywacke, till then a horizontal deposit; whether as a shock or as a pressure, we see clearly, that it could not have acted from east to west, nor from west to east, since in this direction the strata have invariably preserved their primitive direction. The

action must hence have proceeded from the north or the south; exerting itself on a vast system of horizontal layers, in a soft or flexible state, this pressure would drive them back on one another, and force them to gain in vertical height what they lost in horizontal extent. Thence would proceed those varied folds which give the most striking idea of a violent convolution. Geologists who have ascribed this contortion of the strata to the inequality and irregularity of the substances, have not fairly met the difficulty. Dr. Hutton himself and Professor Playfair, who described with so much pains these remarkable phenomena, have not given a more satisfactory explanation than those who have combated their doctrines. For, with a little reflection, we can understand that similar effects could have been produced only by a force acting horizontally, and in a direction parallel to that of the strata; and not as they supposed, by an action directed from below upwards; a species of force which might no doubt raise the beds in a variety of directions, but could not drive them back on each other in a uniform direction. On approaching the bold promontory of St. Abbs-head, the beds of greywacke are seen to resume by degrees their regularity, and finally become large strata, with plane and parallel surfaces. They are now nearly perpendicular.

On the banks of the little streamlet, Fasnet, which runs along a valley amid the hills of Lammermuir, there is a rock of granitic aspect among the strata of greywacke. Mr. Necker describes it minutely, and shews that Mr. Playfair was mistaken in thinking it a genuine granite. It contains more than three elements; it is a syenitic granite of transition, is found in beds subordinate to the greywacke, and affords an argillaceous smell when breathed upon.

On his way from Edinburgh to Arran, Mr. Necker took a cursory view of the scenery of Argyleshire, which seems to have laid a strong hold on his fancy. "Our eyes," says he, "were rivetted to the savage mountains which surrounded us, to the south and west. These elevations, covered merely with a short grass, mingled with a black and melancholy heath, enclosed between them regions more melancholy still. One of them, Glenmolachan, struck us by the extreme solitude and profound silence which reigned in it. This sad scene which appears like the valley of death, descends by a rapid declivity between two mountains. A little brook runs in the bottom, and vanishes from time to time among the purple heather. The approach of night, and the dense clouds which obscured the last rays of the sun, added a new horror to this picture. A black lake placed between bleak promontories and rugged rocks, without any semblance of human culture or habitation, presented towards the north, a pros-

pect more savage than that of Glen-Molachan. Few scenes even in the wildest places of the Alps, amid the eternal glaciers of its valleys and the snows of its huge mountains, have made a deeper impression on me than this first entry into the Highlands. I imagined that I could discover in this wilderness of nature, the secret of the wild and melancholy character of the inhabitant of these mountains, of his superstitions, of his plaintive music, and of his poetry which speaks to the imagination in the strongest figures." His impressions on landing in Arran are thus described: "Some miserable huts scattered up and down at great distances from each other, were the only objects which indicated that this island, barren as it seemed, was not altogether deserted. I had heard of a town of Brodick, where we were to land; I looked for it, but could see only Brodick-Castle. What was my surprise when they pointed out to me, on the sea-shore, four or five little huts rudely covered with turf, difficult to distinguish from the rocks and the heath; and, when they said to me, *that* is Brodick, the capital of the island of Arran. A village of fishermen would have been a superb city compared with this Brodick, which resembled the most wretched establishment of a Lapland horde!"

Mr. Necker was not left long to these unpleasant feelings. The ever-ready hospitality of its people, and the supreme interest of its rock-scenery to a geologist, soon reconciled him to Arran.

The mountains of this island form a confused group. They are not disposed in chains, nor are they separated by long valleys, which, like those of the Alps and the Pyrenees, preserve for a long space, the same direction. The glens, immense hollows in the form of a funnel, divide the mountains into several distinct summits. Some idea may be formed of the conformation of these glens, by viewing in the Alps, the bottom of the valley of Sixt, that of the valley of Louesch, and those enormous chasms, at the foot of the granitic needles of the central chain, from which proceed the beautiful glaciers, that descend into the valley of Chamouni. The Pyrenees offer also similar appearances in the loftiest portion of their chain.

The mountains of Arran are all collected in the northern half of the island. Goatfield, the highest of all the summits, is placed towards the southern limit of the mountain district, from which, southwards, the heights rapidly diminish into moderate hills, and pass latterly into lands of little elevation, which constitute the southern portion. The highest part, as we may naturally suppose, is formed of primitive rocks, while the flat country of the south belongs to more recent formations. Brodick is situated at the limit of these two great mineral masses. The castle is built on rocks of standstone, schistose

clay, and secondary limestone; but a little further north, the micaceous schists and granites commence. If a line be drawn from east to west, through the castle of Brodich we shall have approximatively the geographical limit of the primitive and secondary rocks. A third formation very frequently appears in the southern portion of the island; namely, that of trap, which is found sometimes in rounded cones, standing on the top of secondary hills, and sometimes under the forms of large and extended veins, (whin dykes) which intersect their strata. These veins stretch occasionally even into the primitive rocks, grunstein, basalt, clinkstone, pitchstone, and a peculiar porphyry, constitute the rocks of the Arran trap-formation.

The relative position of the three rocks, sandstone, pitchstone, and clinckstone, which occur near the wood of Brodich, is difficult to determine. The pitchstone seems disposed rather in great detached blocks, than as forming beds or veins. It appears at one time above, and another below the sandstone, which is greatly indurated in its neighbourhood. The clinkstone is also spread up and down, apparently without mutual connexion. Some miles to the south of Brodich, on the way to the larger village of Lamlash, we find a vein of pitchstone of a bottle-green colour, which crosses the road. Its width is eight feet. A rivulet which passes near the road, has laid open a portion of this vein, and has shewn it to be accompanied by a collateral vein of clinkstone, which here separates the pitchstone from the sandstone strata, which compose this platform. On travelling further to the southwards, we come to the hills of Dunfeune and Dundou, the most elevated summits of the promontory which separates the two bays of Lamlash and Brodich. The lower part of this promontory is composed of red sandstone, and of a breccia, having for its basis the same sandstone, which forms the rocks and cliffs on the sea-shore, as well as the lofty hills on which are built the hamlet of Corygills.

On this sandstone there rests a mass of trap of a porphyry with a petrosilex basis, which occupies the summits of Dunpeune and Dundou, exhibiting very regular prismatic columns. Veins of pitchstone and basalt, and beds of greenstone occur in great abundance, in the strata of sandstone and breccia which form the basis of these hills.

Mr. Necker thinks that the trap-rock which caps the above eminences, has been improperly taken by Mr. Jameson for a porphyry with a basis of wacke, from which it differs essentially, in hardness, fracture, and its habitude with the blow-pipe. This beautiful rock is a porphyry formed by three very distinct elements: 1*st*, A homogeneous paste of feldspar or petrosilex; 2*d*, Crystals of quartz; 3*d*, Crystals of feldspar. The

paste is very compact, and of a very fine grain. To the naked eye, its aspect is dull; its fracture appears unequal and earthy; its colour is of a delicate grey. Viewed in a lens, its fracture appears scaly, fine, and its scales are whitish and semitransparent; it has a waxy lustre; is hard; a point of steel, after breaking the small scales, leaves a metallic trace on the stone. It is dense, heavy, and easily frangible. It affords a strong argillaceous odour when breathed upon, and gives feeble sparks with steel. At the blowpipe it whitens, cracks, and finally melts into a white enamel, somewhat vesicular. "It is clear," concludes Mr. Necker, "that this rock has been untruly regarded as having a wacke basis, since it possesses none of the characters which distinguish the latter mineral. He considers it as a new product of the trap-formation. It approaches in the nature of its basis to the *laves petrosiliceuses* described by Dolomieu, in his catalogue of the *Lavas of Ætna*, p. 254, No. CXI; and p. 239, No. XXIII; and by Brochant, vol. ii. p. 627; as also to certain compact domites of the rocks of Mont d'Or and of Cantal in Auvergne. This porphyry occurs at Dundou in the form of pretty regular hexahedral prisms, covered with a whitish crust from decomposition. In the lower part of the hill these prisms are vertical, whilst on its summit they are horizontal. It is remarkable that those of the columns which occupy the culminating point of the trap-cone, are broken through the middle. The country people affirm that this was not the case ninety years ago, and that it was a thunder-stroke which at that period, fell on these rocks, and produced the fractures.

There is a huge bed of green pitchstone, in a cliff of red sandstone, at the foot of Dunfeune, which has a beautiful appearance. This mass, three hundred feet long, and twelve feet thick, presents throughout so uniform and smooth a surface, that it reflects the sun-beams like an immense plate of polished metal. On seeing it shine at a distance, travellers might fancy they beheld one of those rocks of solid silver, with which the abodes of the Genii are decorated in Eastern romance. In this district there is a great number of whin dykes (veins of basalt) which intersect the sandstone and breccia in different directions. They are perfectly similar to those which occur on the opposite coast of Ayrshire. The perfect parallelism preserved by the sides of the vein, or the planes of the basaltic mass, which touches the walls of the sandstone, is very remarkable. Thus many veins or dykes run in a line absolutely straight for several hundred feet, retaining throughout the same thickness. It is also observable, that the sandstone, on approaching one of these dykes, constantly experiences an evident change, both in its colour and consistence. Near the point of contact, it loses its red tint, and becomes harder.

Considerably to the north of Brodich is a Loch Ranza. Here a curious geological phenomenon occurs. The lofty hill which rises to the south of the coast is composed of a talcous schist, whose strata form with the horizon an angle of nearly 70°. They dip to the south-west, that is towards the interior of the island, and rise to the north-east, on the side of the sea. At the foot of that hill, we see issuing from the sea, and rising up against the mountains, beds of secondary limestone which are inclined at an angle of 40°, but in a direction absolutely opposite to that of the talcous schist, since they dip to the north-east, and rise up to the south-west. When we view in profile these two great formations, whose strata, opposed to each other, display their sections above the soil and the heath, we might fancy we saw two embattled troops in assault, with their lances at rest against each other. The inclination of the beds of talcous schist affords an exception to the general remark, that primitive strata rise up against the centre of the chain or group of mountains to which they belong.

The rocks of the western part of Tor-nid-neon to the south-east of Loch-Ranza, display a fine section of the junction of schist with granite. The schist which occupies all the base of that mountain, forms a part of that vast mantle of slaty rocks which envelope the granitic region of Arran. The granite of its summit is placed at the northern limit of that region; it rises far above the schist, and loses itself beneath its strata. The slate on approaching the granite loses completely the appearance and nature of a talcous schist, and changes by insensible gradations into clay-slate, (thonschiefer). The granite of Tor-nid-neon is absolutely the same with that of Goatfield, and of the other granitic mountains of Arran; in large grains, the feldspar being in great rhomboids, which give it a porphyritic appearance, while the mica is black and scanty. The schist dips to the north and rises up to the south, resting upon the granite; and nothing can be more distinct than the junction of the two rocks. There is no transition, no passing of the one into the other; but they are as different at the point of contact, as every where else. It is here that veins of granite of all dimensions may be discovered issuing from the granitic mass to enter into that of the slate; cutting the *folia* of the latter in various directions, and at various angles. In the spot where the vein begins, the granite is in large grains, and differs in no respect from the rest of the mass; but in proportion as the vein advances and becomes more slender, the grain grows proportionally finer; as, if under constraint from the walls of the vein, the elements of the granite had not been able to separate and crystallize, as distinctly as in the other parts of the mineral. There occurs also a remarkable change in the proportion of these elements, according to their recedure from the mass of

granite. At a certain distance from the origin of the vein, the granite ceases to contain mica; further on, the feldspar disappears; and the vein thereafter terminates by a simple thread of compact quartz. All the veins terminate in a sharp point. Near the top of the mountain, the most considerable veins are observed; of which two parallel ones are from three to four feet in length, and one foot wide at their origin. Others are more extended, but narrower; and some are scarcely an inch thick. In general, the straighter the vein the more finely granulated is the granite. The granite of the veins and that of the mountain, form one continuous body; they are separated by no interval, not even by the slightest fissure. The grain and the nature of the veins, at the places where they enter the schist are so perfectly similar to those of the general mass, that it is impossible on viewing the granite, to say where the mass terminates, and the vein begins. Hence it may be safely asserted, that the epoch of the formation of the mass of granite is contemporaneous with that of the vein. But a difficulty occurs when we try to determine the relative antiquity of the granite and the schist; for, in such a predicament, the system of superposition of Werner, is in manifest contradiction with his own theory of veins.

According to the system of superposition, every rock whose beds rest on another rock, is of a more recent origin than the rock on which it reposes. We ought therefore to conclude, that the clay-slate is more modern than the granite. But, on the other hand, the theory of veins teaches us, that the substance of the vein is more modern than the rock which includes it, since the rent must of necessity have preceded the matter which fills it up. Thus, the granite of the veins should be more recent than the slate which contains it. Yet, the granite of the veins and that of the rest of the great granitic mass are obviously identical and contemporaneous; hence, the slate ought to be more ancient than the mass of granitic, on which it lies. These two propositions are evidently contradictory and incompatible. Which of the two is the more probable? Mr. Necker does not hesitate to affirm, however extraordinary it may seem, according to the ideas current among geologists, that the latter is the more probable opinion; and, that consequently *the granite of the island of Arran must be regarded as newer than the schist which envelopes it.* In fact, we may in certain cases conceive how a mass may cover up another without the upper mass being the more modern of the two. But we cannot imagine how the substance of a vein can have pre-existed the rock which encloses it; how a figure cast in a mould, can have existed before that mould was formed. Two other circumstances in the granite veins of Tor-nid-neon serve to confirm this conclusion. 1st. We see clearly that the crys-

tallization of the vein-granite has been influenced by the greater or less width of the fissures; wherever these were too narrow to permit the entire developement of the crystallization, the crystals have remained small and confused; which has not happened in the larger veins, where the grain of the granite is coarser, and the crystals of feldspar and quartz greater and more regular. 2d. We find dispersed through the granite of the veins and that adjoining their junction, small fragments of slate; sometimes even considerable slaty masses completely buried and enveloped in the granite.

The partisans of the system of Werner for a long time refused to admit the existence of similar veins, issuing from a mass of granite, to enter into rocks, which appeared newer in their eyes. They accused those of careless observation, or of being misled by imagination, who first announced these facts incompatible with the theory of their master. Obliged finally to yield to the force of truth, they have contrived several explanations to reconcile these phenomena with the Wernerian theory; and, have often, says Mr. Necker, made the evidence of facts give way to the desire of supporting an hypothesis. It has been said, 1st. that these pretended veins of granite could be nothing but the continuation of the veins of granite, which, after having traversed the mass of the large-grained granite, had penetrated into the schist; and, that it was consequently a granite of a more recent formation than that of the mountain mass, or of the schist itself; a formation which does not exist in mass, but which appears under the form of veins. The observations already presented on the perfect identity of size in the grain of the vein at its origin, with the grain of the granite mass, and on the gradual diminution which the grain of the vein experiences in proportion as it becomes thinner, give a satisfactory answer to the above objection. It may, however, be added that in the granite of Tor-nid-neon, there is not observed those veins of small-grained granite, which are to be found in the granite of Goatfield and of Glen-Rosa. 2d. It has been said that veins of feldspar slightly mingled with mica have been confounded with veins of real granite, (Jameson's Geognosy, p. 108). There exists indeed a mixed rock of feldspar and mica, which, at the first aspect, offers a great resemblance to granite. Such is, for example, the rock described by M. de Saussure, section 1059 of his *Voyages dans les Alpes*, and which forms veins in a foliated petrosilex, near the cascade of Pissevache in the Valais. But the rock of the veins of Tor-nid-neon is a genuine granite; it contains quartz, feldspar, and mica, in the ordinary proportion of common granite, and it is only when the vein is considerably narrowed that these gradually disappear; first the mica, and then the feldspar, when the vein terminates in a slender

thread of pure quartz. 3dly, The granite of the veins, say they, belongs to that species of granite which Lasius and other mineralogists have styled *regenerated granite*. This granite proceeds according to them, from the carriage of the granitic particles by means of rains, which, having detached them from the mass of granite, has dissolved and then deposited them in the clefts of the schistous rock, where these elements, having reunited, crystallized and reproduced a new granite in veins. This hypothesis would have more probability, were the substance in question more soluble in water than are the elements composing granite. But what renders it utterly inapplicable to the present subject is, the circumstance already noticed, of the perfect identity which exists between the granite of the mass, and the granite of the veins; an identity such, that it is impossible, even by the most minute examination, to trace a line of demarkation between the mass and the vein; whence it appears evident, that both the one and the other have been formed at the same moment and in the same way. 4*th*, In order to conciliate the system of Werner with the introductory appearances presented by these celebrated veins, it has been pretended that they were veins contemporaneous with the rock which they traverse. Let us see if the descriptions which the Wernerians themselves give of contemporaneous veins, accords with Mr. Necker's accurate description of the veins of Tor-nid-neon. "The oldest veins in a mountain," says Jameson, (*Geognosy*, p. 236,) "are those composed of nearly the same materials as the strata they traverse, and in which no distinct seam of separation is to be observed between their sides and walls; on the contrary, the substance of the vein is to be observed mixed with, and passing into, that of the rock; and it wedges out in every direction in the mass of rock; thus shewing that it has not been filled from above or below, but is, as it were, a secretion from the rock itself. Such veins are denominated *contemporaneous*, because they appear to have been formed almost at the same time with the rock in which they occur." Mr. Jameson then proceeds to cite Tor-nid-neon, as an illustration. But Mr. Necker says, and we think justly, that the characters which distinguish contemporaneous veins do not agree in any respect with the veins of Tor-nid-neon. In fact, they are not enveloped on all hands with the mass of schist, and they do not terminate on every side in a point, since the largest part of the vein opens itself always wider towards the mass of granite; while they form but a single point at the place farthest removed from this mass. The substance (wall) of the vein, is very different from that of the rock; for the one is a granite composed of feldspar, quartz and mica, the other a slate composed of clay and quartz. The separation between the two rocks is so neat and decided, that very few specimens

may be collected, in which one of the sides of the stone is distinctly slaty, and the other distinctly granitic. There is, therefore, neither passage nor insensible transition. Here are abundance of facts to demonstrate that the veins of granite are not contemporaneous with the schist, but that they are derived from the same mass of granite which forms the centre of the mountain district of Arran. No one will pretend to say, at the present day, that this mass of many leagues in diameter is contemporaneous with the schist which envelopes it, and is only an immense contemporaneous vein. If we abandon the fundamental axiom of geology, that two rocks which differ in their nature and stratification, belong to periods of different formations, it is easy to perceive that the basis on which the science itself reposes is destroyed, and that the edifice must fall to ruins, without the possibility of ever rebuilding it on solid foundations.

" Without disputing about words," says Mr. Necker, " it is enough to have shewn that the rock which constitutes the lowest of the formations of Arran, is more modern than the rock which is immediately placed above it, since, under the form of veins, it has penetrated into the fissures of the superior rock. Hence I have seen with astonishment the learned Professor Jameson refuse entirely to admit the existence of veins proceeding from a mass of granite, to spread themselves in a superposited rock. I cannot believe that he has seen the mountain of Tor-nid-neon, or that he has observed it with all the attention of which he is capable, and with a mind free from prejudice; for then he would not, two several times in his *Elements of Geognosy*, (pp. 110 and 137.) have formally denied so important a fact, and one, at the same time, so evident *."

There are several other places in the island of Arran, where the same phenomenon occurs; for example, in the southern face of Goatfield, in several portions of Glen Rosa, and in the mountains which skirt the south of Glen Sannock. Mr. Necker says, it may be presumed that, wherever the granite touches the mantle of slate which covers it, it penetrates this rock in the form of veins.

But the island of Arran is not the only place of Europe where veins of granite or syenite have been found; for it is to be remarked, that these two rocks are the only ones hitherto observed, which form veins in the incumbent rocks. At the foot of the granitic hills of Dartmoor in Devonshire, Mr. Necker observed, in 1809, large veins of a syenite of red feldspar, white quartz and horneblende, penetrate a schistous greenstone of transition, composed of granular feldspar and hornblende. It is in the bed of the river Elme, a mile and a half north of

* Tom. II. p. 65.

Ivy-bridge, at the base of the granitic mountain called Three Barrow-Tor. In Mount St. Michael in Cornwall, a white granite penetrates a schistous rock; and in both situations, as at Tor-nid-neon, the granite of the veins is quite continuous with that of the mass, and increases in the smallness of its crystals with the contraction of the veins. Mr. Jameson himself has described some in his work on the mineralogy of Scotland. M. de Saussure saw such veins at the Valorsine and Lyons, on the banks of the Saone; and gave a detailed description of them in the twelfth chapter of the second part of his *Voyages dans les Alpes*, Vol. I. p. 530. Lastly, the German mineralogist, Lasius, describes some veins of granite of Reh-berge-Klippe, on the confines of the granitic group, which forms the centre of the Hartz mountains *.

So many analogous observations, made in countries so far asunder, lead us to bestow on a phenomenon, whose generality appears to be proved, more importance than we would have allowed, had it been only once presented as a single exception to a general rule.

Mr. Necker draws the following conclusions. 1*st*. Granite is not in all cases, as has been for a long time believed, the most ancient of rocks. This fact has been lately placed beyond all doubt, by Mr. Von Buch's observations in Norway. He found beds of granite incumbent on micaceous schist, argillaceous schist, and gabro. He has observed also a granite of transition alternating with the greywacke and transition limestone †. 2*d*. The most important deduction is, that there may be, and actually exist, cases where the lowest rock in the order of superposition, may not be the oldest; and where, consequently, the principle on which are founded all the systems of geognosy, and which have been hitherto regarded as an axiom, would lead us into serious errors. It is therefore important, that geological observers should direct their researches towards the points where two rocks of a different nature meet; the junctions, often difficult to discover, almost always concealed by dèbris, vegetation, or the beds of rivers, are nevertheless, in many instances, the places where we may hope to discover the secrets of nature; the spot where we shall find the true *data* for judging of the mutual relations of rocks. The stratiform or schistous rocks, whose beds and *folia* give us evidently the idea of slow and successive deposites, are seldom separated bluntly from each other; the passage of one rock to another is generally gradual and imperceptible. It is not so with rocks such as the granites of plains, syenites, porphyries, which occur in irregular masses, without vestiges of beds or *folia*, and which

* *Berbachtung über die Hartz-geberge*, Vol. I. p. 94.

† *Voyage en Norwège et en Laponie.*

seem of consequence to have a different origin from the former. Their separation from the adjoining strata is complete; a mathematical line often divides one whole mountain, one rock, one specimen, into two perfectly distinct portions, formed of rocks of a different nature. It is there we ought to follow the line of junction, and to study carefully the appearances of the two rocks.

The promontory of Drumodoon, in Arran, is a most picturesque geological object. It is a hill of sandstone, terminated at its summit by a range of immense prisms of the trap formation; or a bed of feldspar porphyry resting on horizontal beds of sandstone. Mr. Necker shews that Professor Jameson has fallen into the same mistake in assigning this porphyritic rock a basis of wacke, as with respect to Dunfeune. A large vein of greenstone intersects from top to bottom the bed of trap porphyry. The two rocks seem little changed at their line of junction.

It might be imagined that the great difference of height between the mountains of Scotland and of Swisserland, would prevent all comparison between the aspect of the two countries; but this is not the case. Mr. Necker observes, that although the Scotch mountains are much less raised above the level of the sea than the lowest of the Alps, yet as the latter rise out of table land or valleys, of considerable elevation, while the former have their bases at the level of the sea, there is really less difference of height in the eye of an observer, than one might *à priori* expect. "If our views of Swisserland," adds he, "present a whole, more vast and more striking, with a grandeur and majesty no where else to be found, our views in Scotland are perhaps more picturesque, taking this word in its true acceptation, that is, they present landscape subjects more suitable to the painter, details of greater variety and grace *." Scotland has not, like Swisserland, indeed, those mountains covered with eternal snow, those granitic peaks so bold and light, which, by the beauty of their outlines, and the contrast which they make with the lively verdure of the valleys, give to all the *distances* so striking an effect; yet she has, in compensation, lakes interspersed with islands of every form and dimension; she has the Atlantic Ocean, its isles, and inland gulfs, which afford to the ground of the landscape a peculiar beauty.

From Arran Mr. Necker returned to Edinburgh, making a passing survey of the fertile island of Bute. He then set out on his laborious voyage through the Hebrides, in the course of which he discovers equal spirit and intelligence. He visited Staffa twice; spent some days between Coll and Tiree; no-

* VII. p. 208.

ticing in the former the enormous veins of feldspar which intersect in all directions the strata of gneiss, of which the whole island is composed. He regards the two minerals as contemporaneous. The strata run from north-east to south-west. He observed in the southern part of Coll, three veins of basalt in the gneiss; two of which cut the strata obliquely, and the third is parallel to them. The gneiss has experienced no change whatever near its point of contact with the basalt. One of the oblique veins presents a phenomenon not uncommon; the grain of the basalt is finer near the sides of the vein, than in the middle. The two other veins assume at the junction, and even to the distance of two inches from the walls, a slaty aspect, dividing into thin plates parallel to the breadth of the vein. The rock of the curious columns of Scaur-Eigg, Mr. Necker says, has been improperly named a porphyry with base of pitchstone by Professor Jameson; the substance which forms the base of this rock seems to differ essentially from pitchstone, in lustre, texture, and especially hardness. He considers it as a new substance, or as one at least which has not been described hitherto with its true character. He calls it lithoid obsidian, to distinguish it from the vetreous variety. At the foot of the porphyritic ridge of the Scaur, he found basalt in tables, alternating with basalt in irregular prisms, down to the sea-shore. On the coast to the south-east two great veins of vetreous obsidian were observed to traverse the basalt. Nothing has more the appearance of fracture and lustre of glass than this rock. The junction is very sharp; a mathematical line separates the obsidian and basalt, which are not confounded together, and never pass into one another by insensible shades. The basalt does not seem to have undergone any notable modification in the neighbourhood of the obsidian veins. The base of the island on the east and south sides, is an earthy basalt or tender wacke. Mr. Jameson asserts that the lowest strata are composed of limestone and schistous clay. These are of course under the wacke which alone is apparent in the greater part of the island.

We shall not follow Mr. Necker through the rest of the Hebrides, especially as we possess in this country already Dr. Macculloch's excellent work, of whose general accuracy the Genevese geologist speaks in the highest terms. On his return across the main land of Scotland, our author notices the downs or moving sands on the coast of the southern Murray Firth. The inhabitants, mistaken as to the cause of so formidable a phenomenon, do nothing requisite to hinder its disastrous effects. They regard these masses of sand as produced by extraordinary inundations of the sea, and by rare and accidental subversions of the soil. But it is not to so uncommon causes that we must have recourse to

explain the increasing accumulation of sands on the shores of Morayshire. The constant and ordinary action of the tide, sun, and wind, is its true cause, and hence this accumulation being susceptible of indefinite increase, it becomes a matter of the first urgency to apply a remedy to it; which urgency is not sufficiently felt by the people of that country. It is obvious that, as they cannot hinder the sea from throwing the sand on their shores, they ought simply to try to prevent the wind from carrying this sand inland. Now, there is only one method of effecting this; namely, the fixation of the downs. In order to fix in their place these hills entirely composed of volatile grains which may be wafted by the wind, we must imitate what nature does in like cases, and clothe the surface of the downs exposed to the prevailing wind, with such creeping plants as live in the sands, and form a network capable of retaining the loose and light particles. An act of parliament has already prohibited the pulling of the *Arundo arenaria*, (sea mat-weed), which grows on the downs, and serves to consolidate them. But this is not enough. We must aid nature, and by repeated sowing, multiply the existing plants, and add new species, such as elymus arenarius, (sea lime-grass), and especially the ulex Europæus, or furze, called in Scotland, whins. A complete development of the precautions to be taken in this kind of culture, is to be found in the interesting memoir of M. Desmarets on the downs of the departments of the Gironde and the Landes, which forms a part of the dictionary of physical geography in the *Encyclopédie Méthodique.* The memoir of M. Decandolle, on the fertilization of downs, inserted in the 12th volume of the *Annals of French Agriculture*, deserves also to be consulted. This learned naturalist has occupied himself not only with the problem of fixing downs; but he has also resolved the more important problem, of changing these heaps of sand into productive and fertile soils. He indicates with this view the kinds of plantations most suitable to sandy soils, and he enters into interesting details on the precaution indispensable to success in such undertakings. The shores of France on the Bay of Biscay present on a greater scale the same phenomena of advancing downs as the coast of Morayshire. The same means by which they have succeeded in arresting on some points of these shores, the invasion of fertile fields by moving sands, the change of the beds of rivers, the formation of ponds, and finally, the burying of edifices, trees, and whole villages, might undoubtedly be employed with success in Scotland.

We shall conclude our analysis of this very interesting work by a general view of the geological structure of Scotland. This country presents four great classes of formation; namely, 1*st*, Primitive formations; 2*d*, Intermediate; 3*d*, Secondary; 4*th*, Alluvial. The first class occupies all the great mountain

masses of the Highlands, or the Grampian hills; the second, the hills of the southern districts of the Lowlands, or the Lammermuir hills, as also a narrow fringe in the southern limit of the Grampians; the third fills up the bottom of the large valley or basin laid open between these two mountain masses, and forms the low countries on the shores of Moray Firth, and on the eastern coasts of Sutherland and Caithness. Lastly, the fourth is found every where as a superficial stratum, in the bottom of the valleys, and on all the low grounds of Scotland. The direction from north-east to south-west, which the bands formed by these different districts uniformly affect, is as distinct as may be. It is determined by the general direction of the strata, equally from north-east to south-west, a direction which determines also that of the entire chain, and of its different lateral links. Thus the grounds are placed in succession, parallel to the axis of the greatest heights, or to the physical axis of the chain. As to the general inclination of these masses, it must be evidently in a line perpendicular to the general direction, whatever be in other respects the partial inclination of the beds and strata which compose them. Now, since the secondary formations rest on the intermediate, (transition), and these on the primitive, and since the primitive occupy the north-west of the country, whilst the passage of the oldest regions into the newest, takes place in advancing from that point to the south-east, we may thence conclude that the general inclination of each district is towards the south-east. Thus we shall represent the great classes of formations, as forming each an immense mass, or bed, running from south-west to north-east, and dipping towards the south-east. From this position, however, we must except the class of secondary formations, because here at least it has been manifestly deposited in a basin; and, although it participates on the borders of the basin, in the general direction, it has not any determinate single inclination, because it rises up on every side, at these borders.

The *primitive formations* occupy all the space included between the Pentland Firth, which separates the Orkneys from Scotland, and the southern limit of the Grampians, with the exception of a secondary band on the coasts of Caithness and the Moray Firth. Three principal formations, or three very distinct masses, occupy this space; these are gneiss, mica-schist, and schistous chlorite which here holds the place, and represents the formation of clay-slate. The topographical limits between these three formations cannot be fixed with perfect precision, because there is a gradual transition among them. Yet we may say that the longitudinal valley of the Caledonian canal presents pretty nearly the limit between the formation of gneiss and of mica-schists, the former predominating in the

mountains to the north of this valley, the latter in those to the south. The limit between this last formation and that of the schistous chlorite is less distinct, but it may be placed approximatively on a line directed from S.W. to N.E., beginning at Tarbet, on the western coast, passing along the south side of Loch-Fine, through Glen-Dochart, Loch-Tay, to Killicrankie, and thence proceeding towards a point between Stonehaven and Aberdeen, on the eastern coast, where it terminates. Mr. Necker doubts whether the fundamental granite of Werner be visible in Scotland. The gneiss shews itself well characterized only in the most ancient parts of the formation, which, by inspecting a map, we shall find, on the above principles of general direction of strata, must be in the Hebridian Isles, where the three elements of that rock are in nearly equal proportion. On the mainland, on approaching the mica-schists, the feldspar diminishes, the mica augments, and the rock assumes a more foliated structure. Serpentine, amphibolite, diabase, primitive limestone, mica-schist, a species of clay-slate, and euphotide, (gabbro of the Italians and Von Buch,) form subordinate beds in the gneiss. Of all these subordinate beds none plays a greater part in this formation than the quartz rock, whether compact or granular, sometimes pure, sometimes mixed with mica, and occasionally with feldspar; the quartz rock is so abundant in this district, that it constitutes by itself nearly the country of Assynt and Groïnard, on the north-west coast of Scotland, forming mountains which are nearly 3,000 feet high. Its vertical beds resisting decomposition much more than the gneiss, present in their high summits the forms of towers, needles, peaks, and sharp ridges, which resemble the protogine of the Savoy Alps. Mr. Necker conceives that Dr. Mac Culloch should have ranked his primitive red standstones with quartz rocks, and he refers, in justification of this criticism, to M. de Saussure, Brochant, and d'Aubuisson. The formation of gneiss is characterized by a great abundance of granitic veins, which intersect the strata in different directions, and vary infinitely in length and thickness. The only district in this formation which has hitherto presented metallic veins is at Strontian. In the *mica-schist* formation are placed the highest mountains of Scotland, occasionally rising to 3,600 feet.

The formation of schistous chlorite, or chlorite slate, which term Mr. Necker substitutes for clay slate, has subordinate rocks very distinct from those contained by the older formations. Such are those mixtures of feldspar, quartz, and talc, or chlorite, which according to the greater or less developement of crystallization, assume so many different aspects, but which, however, should all come under the denomination of *protogine*. The breadth of the stripe of ground occupied by this formation is not very considerable. It extends from the line indi-

cated as the southern limit of the mica-schist formation to the base of the Grampian mountains. This breadth is not equal over all the extent of the stripe, for whilst at the west it occupies the whole length of the peninsula of Cantyre, on the east coast, it is scarcely a few hundred fathoms broad.

The *greywacke formation* is the only one of transition which is found in Scotland, but it occupies so large a space, and exhibits such remarkable phenomena, as to merit the mature consideration of geologists. The limit where the greywacke formation commences is a line drawn from N.E. to S.W., along the foot of the Grampians, from Stonehaven to Renton, a village situated between Dumbarton and Loch-Lomond, and this formation stretches considerably into England, where it constitutes a part of the mountains of Cumberland and Westmoreland. In the midst of this mass of greywacke we have a discontinuity in the basins of the Forth and Clyde, which is filled up by the secondary rocks, of red sandstone and coal sandstone, which again are traversed, and often covered by basaltic or trap masses. These same secondary beds do not, however, fill up completely the bottom of the basin, but we observe issuing from the midst of the sandstone, and rising to heights more or less considerable, stratiform masses, composed of rocks subordinate to the greywacke formation, which constitute chains of hills, hillocks, or insulated rocks of no small magnitude. The puddingstones are the oldest, and consequently the lowest part of the greywacke formation, and they constitute the northern borders of the basin of red sandstone. Secondly, we have the greywackes, properly so called, which form the southern borders of the basin, namely, the long chain of the Lammermuirs, and which form in Scotland at least the newest beds of the formation which bears their name. Lastly, we have beds subordinate to the puddingstones, or intermediate between these and the greywacke, beds which form protuberances that rise suddenly from the bottom of the basin when the red sandstone is deposited. The composition of the puddingstone is like that of the greywacke, fragments of primitive rocks, under the form of pebbles or rounded blocks, united by a cement of granular micaceous quartz, sometimes of the nature of clay-slate, sometimes by a reddish ochry clay, and rarely by a calcareous cement.

Beds often pretty thick, but not divided into strata, of a compact feldspar are found interposed between the rows of puddingstones and sandstones; and as they are less easily attacked by the weather, they resist longer, and form elevated ridges, such as the mountains of Sidlay, Ochiels, and Campsie, which run in a chain from south-west to north-east, parallel to the general direction of the formations, and to that of their

own beds. The inclination of the beds is generally like that of the surrounding districts to the south-east. South of the foot of these hills commences the formation of red sandstone; northward are the beds of the greywacke formation. The great chain which extends from the mull of Galloway, across Scotland, to St. Abb's-head is greywacke, properly so called, and greywacke slate. The fragments sometimes attain the size of a hen's egg. The subordinate beds common in this formation, such as the clay, alum, and siliceous slates, as well as a black-coloured limestone are found frequently in these mountains. The greywacke acquires in this country great importance by the number, extent, and richness of the metallic veins, which abound in this formation. The lead hills' district at 2,300 feet of elevation, towards the centre and on the northern face of the chain, exhibits the most considerable mining operations of Scotland; lead, copper, iron, and manganese occur in abundance.

The red sandstone forms in Scotland the inferior beds, whilst the coal-sandstone is superincumbent on it; the former of which exhibit great disorder in their direction, as well as in their inclination. Yet on the whole, they form a concave stratification, rising up on all sides upon the edges of the containing basins.

With regard to *local and independent formations*, it appears certain, that Scotland is hitherto the only country where such districts have exhibited in a clear and evident manner, those important geological characters, which distinguish them from the ordinary class of formations, although these mineral masses have their counterparts in other countries. The characters of the rocks of this class are: 1*st*, They are never stratified, but occur under the form of amorphous masses, prisms, balls, and tables of little length. 2*d*, They are never interposed between two beds of foreign rocks, so as to form between these, a bed of indefinite length, constantly parallel to the strata. 3*d*, They never conform to the stratification, whether general or particular, of the formations and rocks which surround them. 4*th*, They penetrate into the middle of the surrounding rocks, filling up their rents, in the form of veins, which veins sometimes evidently proceed from a mass similar to themselves; sometimes they appear insulated, although never at a very great distance from a similar mass. Hence it is obviously very difficult, with respect to the independent formations, to seize exactly their relations of antiquity, not only with the formations which adjoin them, but also with other independent formations. They may be divided into three orders; 1*st*, Those in which granite forms the predominating rock; 2*d*, Those characterized by the abundance of porphyry; and 3*d*, Those which consist in a great measure of greenstone-trap and basalt. These divisions,

moreover, in the order in which we have named them, represent pretty nearly the geognostic position of these masses, in reference to their antiquity; for the trap-formation, penetrating the secondary formations in veins, is more recent than them; whilst the granitic and porphyritic formations are themselves traversed by trap veins, and do not pénetrate beyond the transition beds. Lastly, the veins of porphyry traverse the granitic masses, which shews that the latter are the most ancient of the particular formations.

Numerous veins of granite traverse the gneiss, between the Bay of Laxfiord and Cape Wrath. The Ord of Caithness, a mountain 1,200 feet high, is a species of graphic granite, rising out of the gneiss district. But it is in the mica-schist formation that the most considerable granitic masses are to be found. Thus the district of Braemar, bounded to the north-east, by the valley of the Spey, to the south-east by Glen Tilt, forms a great granitic island, amidst the mica-schist and its subordinate rocks, across which veins of granite extend to a great distance. Other similar groups, although less extensive, are seen in the county of Aberdeen, between the rivers Dee and Don, and on the banks of the Linné-Loch and Loch-Leven in Invernesshire. The prevailing mass of this last group is a sienitic granite. Near Loch-Rannoch there is a mass of granite, eight miles long; and the southern foot of the Coriarich mountain is sienitic granite. Masses and veins of granite are found on the borders of Loch-Ness. The granite of the island of Arran rises evidently in the middle of mountains belonging to the chlorite-slate formation. But what is most remarkable, is to find masses of sienitic granite, which pierce their way across beds of the greywacke formation, as is observed in the county of Kircudbright, in the south of Scotland. Three districts, or islands, separated from each other by beds of greywacke, are occupied by granite-formations. The first mass is on the banks of Loch-Doon, the second on those of Loch-Ken, and the third on the sea-shore, between the mouth of the rivers Nith and Urr. Here Mount Criffel, a granitic mass, is nearly 1,700 feet high.

Ben-Nevis and Ben-Cruachan, the two greatest mountain masses in the island, are composed of porphyry.

The rocks of the *trap-formation*, relative to their mineralogical composition, may be divided into three classes. The first is the trap-greenstone, of which we have an example in the rock of Salisbury-craig; 2*d*, The basalt, like that of Arthur-seat, and of Edinburgh-Castle; and lastly, the trap-porphyries and feldspar rocks, such as the porphyries, pitch-stones and obsidians of Arran and Eigg. The two banks of the Firth of Forth up to Stirling, are characterized by the abundance of greenstones and their wackes. In the district

of Clyde, comprising Bute and Arran, the feldspar porphyries the clink-stones, the veins of pitch-stone, and the accompanying feldspar basalts seem to predominate; notwithstanding which, veins of green-stone and masses of genuine basalt occur. Lastly, the two principal trap districts of the Hebrides, those of Mull and Sky present scarcely any other thing than great masses of basalt with compact feldspar; amygdaloidal basalt, basaltic wacke, with a small quantity of obsidians, green-stones, and porphyries.

On the two banks of the Firth of Forth, the crests of green-stone, which may be considered as portions of veins, rise every where above the coal-beds, as at Salisbury-craigs; on the sea-shore between Leith and Queen's-ferry on the south-side; and the islands of Inch-Colm and Inch-Garvey at North-Queen's-ferry; and at Aberdour, Burntisland, and on the north shore. In this same district, the green-stone occurs also under the evident form of veins of a great length. From these large veins much narrower ones proceed, which spread themselves in the neighbouring beds. According to Mr. Necker, it is similar narrow or thin veins which by sections, perpendicular to their direction, exhibit in the quarries those deceitful appearances of irregular insulated masses of green-stone, in the middle of the coal strata. "Mr. Jameson," says the Genevese geologist, "has described and figured these pretended insulated masses, in the first volume of the *Edinburgh Philosophical Journal.* He endeavours to derive from them a support to his new system of contemporaneous formations, to which he appears to give every day more extension. As soon forsooth as a series of different rocks presents some obscurity in its position (*gissement*), some appearances in their relations with each other which cannot be easily explained on the Neptunian hypothesis, he cuts through the difficulty slap-dash, by declaring all these rocks to be of contemporaneous formation. It is thus according to him that we find in the county of Caithness, masses of syenite, granite, lime-stone, conglomerate, and sand-stone, all simultaneously formed." (*Edinburgh Philosophical Journal*, Vol. II., p. 377). "At the Cape of Good Hope, gneiss, clay-slates, rocks of quartz, and of granular quartz, improperly called red sand-stone, are traversed by granitic veins. Conglomerates and sand-stones, probably secondary, are close by, but the superposition and relations of these rocks to one another appear sufficiently obscure, yet every thing is quite clear to Mr. Jameson, who regards all these rocks as formed simultaneously in the position where we now find them." (Ibid, Vol. I., p. 283).

Lastly, the pretended insulated masses of green-stone occur in the hill on which Edinburgh stands, and they are surrounded by beds of coal sand-stone, with palm impressions, which

alternate with beds of slate-clay bearing impressions of ferns and shell petrifactions. Now, that all these beds and the masses of green-stone could have been formed simultaneously and in the same manner, it is impossible to admit. For, if we can conceive that veins formed of the same elements as the rock which they traverse, may be contemporaneous with it, if we should even go the length to allow that the *folia* of certain schists, and the strata of certain primitive rocks, may be owing to another cause than successive deposits, and thereby to grant that a vein which is at the same time in contact with several of these *folia* and these *strata*, may still be contemporaneous with the whole; this admission cannot surely be made as to a mass composed of beds of coal-sandstone and slate clay, where each bed bears the most manifest characters of a particular deposite. What can be the meaning of a vein contemporaneous with a mass, all of whose parts are not contemporáneous with one another? Such a system, besides, had it the support of all the probabilities that militate against it, would not be less hurtful to the advancement of geology, since it would invite the geologist to content himself with vague observations, and would subject him to mistake every instant false appearances for realities. Finally, if those mineralogical distinctions established with so much ease, by so many skilful observers; if those laws of the succession of formations, verified on the most distant points of the globe; if all this edifice which has taken the toil and talents of so many eminent men to elevate; if all this science of geognosy ought definitively to lead us to recognise in this globe, but a confused mass of rocks piled up without any order, and all formed simultaneously; we must needs abandon a study henceforth unworthy of the name of a science, and banish geognosy among those vain amusements of a steril curiosity little formed to attract the notice of philosophical minds.

We here quit our intelligent and cheerful guide among the rocks of Scotland, with regret and gratitude; feelings in which all our geological readers will no doubt sympathize, notwithstanding the unusual length of this analysis. The disquisitions on the manners, the civil and political institutions of the country, we have passed over, for obvious reasons; but they are all written in a just and generous spirit. They will prove at once gratifying to the natives of that romantic region, and instructive to strangers. The style is worthy of the kinsman of Madame de Staël, and of the son of her elegant biographer, Madame Necker de Saussure.

Art. XVII. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. IX.

i. *A Reply to Mr. Baily's Remarks on the Nautical Almanac.*

In a volume of Astronomical Tables for the year 1822, which has been privately printed, but extensively circulated, for the convenience of practical astronomers, Mr. Baily has introduced several severe remarks on the *Nautical Almanac*, which have been considered by those who are better acquainted with Mr. Baily's general merits, than with the actual state of the facts in question, as requiring some confutation or explanation.

1. "Perhaps," says Mr. Baily, in his preface, "there are many practical astronomers, who will agree with me in thinking, that some of these tables would form a valuable addition to the annual volumes of the *Nautical Almanac*. It may perhaps be urged, that, as the *Nautical Almanac* is intended for *nautical* purposes only, its original design is fully answered in its present state. This, however, I conceive to be erroneous; since its very title implies another, and a "more important" object. It has, indeed, long exceeded the limits hitherto supposed to be requisite for mere nautical purposes; but it falls far short of what is now annually required for the uses of astronomy. Some few improvements and additions have, indeed, been recently made; but much more must be done, before it can excel (or even rival) the similar productions which annually issue from the press at Paris, Berlin, Milan, or Vienna."

Now, if Mr. Baily meant to give the preference to these publications, because they unite with the merits of an almanac those of an astronomical journal and review, it would have been more candid to have stated this objection in a separate form, and to have demonstrated how far it was incumbent on the Board of Longitude to become competitors with the publishers of the numerous and valuable scientific journals which already exist in this country, and which afford a facility, not easily obtained on the continent, of making public many such memoirs as might become proper articles in an annual appendix of the *Nautical Almanac*, if it were thought proper to encumber the volume with a number of pages which, to ninety-nine out of a hundred purchasers of the work, would be totally useless.

Such memoirs as are frequently found in the "*Additions*," if they were presented to the Board of Longitude, would naturally be transmitted by them to the Royal Society: and any fragments which fell from the tables of these epicures in science, might perhaps be accepted with becoming gratitude by some other collectors, who, either individually or collectively, might

assist in preserving from oblivion all that deserves to be remembered, and a great deal that might as well be forgotten.

But with regard to the general *accuracy* of the computations, and the impression, it is already acknowledged throughout Europe, that the *Nautical Almanac* is the most correct of all the ephemerides which are intended for nautical uses. Let Mr. Baily only turn to pages 371, 372, and 373 of the *Connaissance des Tems* for the present year, and see how those pages are filled, and from whom the materials were received; let him consider that the *Nautical Almanac* is always published six months before the *Connaissance des Tems;* and let him examine the eclipses of the fourth satellite of Jupiter for 1824, and, after this, let him pronounce a distinct opinion upon the comparative accuracy of the two publications. Perhaps he will say, as Mr. Arago has done on the same subject, "*S'il y a du mérite à servir d'exemple aux autres, il y en a peut-être plús encore à ne pas s'en vanter soi-même;*" but Mr. Arago, even in a moment of accidental irritation, did not attempt to disprove the general superiority of the *Nautical Almanac*, as a correct ephemeris.

2. Again: "Is it not mortifying to reflect, that, since the time of Halley, (the contemporary of Newton,) this country has not produced a single astronomical table: that although the Royal Observatory at Greenwich has been established nearly one hundred and fifty years, its observations since the time of Flamsteed, have been of no essential advantage to the world, until they have passed through foreign hands, and been returned to this country in the shape of tables, formed for various purposes by the successive labours of MAYER, DELAMBRE, LALANDE, BURG, BURCKHARDT, and BESSEL? Look at the various columns which compose the monthly pages of the *Nautical Almanac*:—there is not a single article of English origin:—they are *all* deduced from tables which have been formed by some of those authors to whom I have just alluded."

Now, in the first place, it might have been expected that a person so conversant as Mr. Baily with the history of astronomy throughout all ages, and so jealous of the honour of his country, would have known, and would have recollected, that the *first* essentially *accurate tables* of the *moon* were fairly produced in this country by Mr. Charles Mason; and he might have read, in a late memorandum on the subject of the lunar tables, that the actual introduction of these tables, with all their equations, was *delayed*, rather than *expedited*, by the respect of Dr. Maskelyne for the computations of the great mathematicians of the continent. It is true that Mason followed the forms already indicated by Euler and Mayer: but without the theory of Newton, and the observations of Bradley and Maske-

lyne, the theorems of Euler and the tables of Mayer would probably have had no existence, notwithstanding the inducement held out by the British government, which so essentially encouraged those mathematicians in their pursuits.

But even if Great Britain had never produced the first good tables of the moon, or had never produced any tables at all, good, bad, or indifferent, that required much mechanical labour, would the fact be a subject for any great lamentation or mortification? Or is it to be regretted, that the talents of the English mathematicians have been otherwise employed than in mere numerical computations? In France and in Germany may be found a number of ingenious and industrious men, whose activity and frugality renders them peculiarly adapted for the more laborious departments of literature and science, and who are satisfied with a moderate remuneration, in the shape of a professor's salary, or an academical pension; and although they could seldom very conveniently give up their time gratuitously to the good of the public, they are satisfied with the consciousness of being useful, without the ambition to be brilliant. In Great Britain, the whole machinery of politics, of literature, and of science, is carried on by volunteers: their recompense is in the credit that they derive from their pursuits: they have no motive for labouring in the mere mechanical execution of the inventions of others; their ambition is directed to higher objects: the beginning and ending of all scientific works, as well as of works of art, are the most arduous; the middle may be left to the hand of the mere labourer: and it is neither a disgrace to Great Britain to profit by the labours of the foreigner when labour only is wanted, nor a dishonour to the foreigner to fill up the void, which has been accidentally or temporarily left, by the talent and patronage of Great Britain. As soon, however, as the present tables shall have been found inadequate to the purposes required of the *Nautical Almanac*, it will become the duty of the Board of Longitude to seek for others: but there is no reason why their means should be dissipated upon the construction of tables, subservient to mere curiosity, and to the contingent improvement of some of the details of mathematical astronomy.

In the present embarrassed state of the finances of Great Britain, a certain degree of Machiavelism seems indeed to be at least as essential to a Mæcenas as to a Minister; and even if, instead of offering a scanty pittance to a few computers and comparers, who can neither accept it with gratitude nor refuse it with prudence, and leaving all those, who accomplish any thing truly great, to be recompensed by the enviable enthusiasm of their own self-devotion, and the delightful dreams of posthumous fame; even if the situation of the country made it justifiable to expend, in the *immediate* encouragement of science, one half

as much as is so applied by our continental neighbours, we should probably have a much greater abundance of scientific materials than we have hitherto produced: but it is very questionable whether the increase of quantity would be of any advantage, without a commensurate improvement of the quality of the product.

3. P. iii.—" When Dr. Maskelyne edited the *Nautical Almanac,* he used to insert a copious list of such conjunctions of the moon and stars as would probably be attended with occultation, a practice which has unfortunately been discontinued for some years. In the *Nautical Almanac* for 1824, however, something like the same object has been revived in another manner. Twenty-three of the principal stars in the moon's path have been selected, the times of their conjunction with the moon are given, and the elements for computing the expected occultations are annexed. The present wants of astronomers require, however, a more ample list. Since this sheet was put to the press I have seen a letter from a very distinguished navigator, wherein he states that occultations of fixed stars by the moon, as far as the fourth magnitude, are easily observable at sea, and he regrets that no notice is taken of them in any nautical work."

It ought perhaps to have been stated by the editor of the *Nautical Almanac* for 1815, which was printed in 1811, that the conjunctions of the moon with the smaller stars had been left out by the express order of Dr. Maskelyne, in conformity with a resolution of the Board of Longitude. It is, however, remarkable enough, that there was a mistake of many minutes in the longitudes of several of the larger stars communicated at the same time by Dr. Maskelyne to the computers; and yet that no practical astronomer had discovered the errors which pervaded the conjunctions inserted from 1815 to 1821, a sufficient proof that little or no use was made of these conjunctions for actual observation. With respect to the stars " selected," that is, by the moon herself, for 1824, it is unfortunate for Mr. Baily's censure that they should comprehend precisely *all* those which the " very distinguished navigator" has found to be *observable at sea,* and surely to insert any others would lead to an endless waste of time, and paper, and printing.

4. P. 11. *Note.*—" It may be useful here to state that, although the quantity 0,″31 has been added to the mean right ascensions at the end of the *Nautical Almanac* for 1823, yet the apparent places, in the same work, are computed without that addition, a circumstance which ought to have been noticed. Moreover, it is not mentioned from what catalogue the apparent north polar distances are calculated, which is also equally necessary, since they are really of *no use* without that information."

The page of stars is generally sent to press after all the rest

of the *Nautical Almanac* has been printed off, and if the alteration had been material, it would have been right to have noticed, in that page, that it had not been applied to the computed apparent places; but the whole almanac appearing under the authority of the Astronomer Royal, it could not be necessary to state that the places of the stars were immediately deduced from *his catalogues*, and from *his observations.* But it would perhaps be as well hereafter to prefix the mean place for the year to the apparent places. With respect to the remarks in p. xix, the editor of the *Nautical Almanac* would think it unbecoming to interfere in any manner with a catalogue furnished officially by the Astronomer Royal, upon the basis of his own observations; and he would not hesitate to admit still greater fluctuations from the mean determination of former years, if they were supported by such authority.

5. P. xxii.—"Table XI contains a list of all the eclipses of Jupiter's satellites, marked as visible at Greenwich, deduced from the *Connaissance des Tems*, for 1822, by deducting the difference of the meridians, or 9′ 21″. The times of the eclipses in that work have been computed from Delambre's *new* tables, published in 1817. (*Note.*—The Commissioners of the Board of Longitude have deferred the use of these tables till the year 1824, a period of seven years from the date of their publication. This is nearly fulfilling the injunction of Horace, *nonumque prematur in annum.* It certainly gives ample time for the detection of any error.) I know not from what tables those in the *Nautical Almanac* have been computed (the laudable custom of informing the public on these points having been for some years omitted,) but there is so striking a difference between the results in the two works, that I thought it might be acceptable to the practical astronomer to have them presented at one view. The differences amount, in some cases, to 2′ 10″. If the computations in the *Nautical Almanac* have been made, as formerly, by two separate persons, and should prove incorrect, it is singular they should both have fallen into precisely the same errors."

In the Preface to the *Nautical Almanac* for 1823, it is observed, that "the tables of the planetary motions, which have been employed, are chiefly those which are printed in the third volume of Professor Vince's *Astronomy*," and the eclipses of the satellites are not included in the exceptions to this remark. The fact is, that these tables were actually employed, with such additions as were required for bringing them down from 1820 to 1823, and these were furnished, at the request of the Astronomer Royal, by the late Mr. Crossley, and by a well known German mathematician and geographer, conjointly. Mr. Bailey may well think it singular that two separate persons should agree in falling into the same errors, and that these errors

should always be on the same side, with respect to the third satellite; but it is still more singular that it should not have occurred to him, that this last circumstance sufficiently proves that the error could not have originated with the computers. Whatever may have been the source of this discordance, it was known to the editor of the *Nautical Almanac* long ago. But the error is comprehended within the limits of probable accuracy, assigned in the explanation, p. 168, and even if this remark of Dr. Maskelyne be thought somewhat obsolete, it will not be denied that Delambre's own authority is of some weight; and he says expressly, in the Introduction to his new tables, p. li., " *On verra Messier et Méchain, dans la même ville, et presque dans le même quartier, tous deux munis d'excellens instrumens et d'une vue excellente, ne s'accorder cependant qu'à quelques minutes près sur la même éclipse. Il est évident, de même, que pour les différences des méridiens,* ON NE PEUT SE FIER QU' AU PREMIER SATELLITE; *les autres ne sont guère bons qu' à éclaircir quelque point de physique céleste; le* PREMIER SATELLITE EST LE SEUL QUI PUISSE ETRE VRAIMENT UTILE *aux astronomes et aux géographes.*" It was then amply sufficient to have LOST NO TIME in procuring copies of the new tables for the future use of the computers, who are always expected to be five or six years in advance with their computations; nor could it be necessary to reject the computations already completed, while it was well known that their results could never be employed for NAUTICAL PURPOSES, at least in the present state of the art of observing at sea.

6. P. xxiii. " * The telescopes *proper* for observing the eclipses of Jupiter's satellites, are common refracting telescopes, from *fifteen* to *twenty* feet." So says the *Nautical Almanac,* but I much doubt whether any one of the Commissioners of the Board of Longitude ever saw a telescope of this kind; nor do I think there is such a thing in existence. How absurd, then, it appears to *recommend* the use of them, and thus *mislead* (as I know it has done,) those entering the career of science."

Now, in the *Nautical Almanac* for 1823 and 1824, the explanation is expressly attributed to " the late Dr. Maskelyne," p. 161, so that it becomes merely historical, with respect to the minute description of the instrument to be employed. But in order to avoid any possible misconception, the paragraph in question is printed thus:

" The telescopes proper for observing the eclipses of Jupiter's satellites, are " common" refracting telescopes from 15 to 20 feet; reflecting telescopes of 18 inches, or two feet focal length, and telescopes of Mr. Dollond's construction, with two object glasses, from 5 to 10 feet; or, which are still more convenient, those of 46 inches focal length, and $3\frac{2}{3}$ inches aperture, constructed with three object glasses, which are as ma-

nageable as reflecting telescopes, and perform as much as those which he makes of 10 feet, with two object glasses." The marking the word *common* in this manner, sufficiently implies that the telescopes in question are now become *uncommon.*

7. P. xxv. " For the *mean* obliquity of the ecliptic, the Preface of the *Nautical Almanac* certainly renders the Astronomer Royal responsible. The equations are computed from Delambre's tables."

8. P. xxvi. " * Ephemeris, by Schumacher. A regular supply *should have been* sent over to this country in the first instance." It not only *was* so sent, but was advertised for sale by the bookseller to the Board of Longitude, long before the appearance of Mr. Baily's astronomical tables and remarks, which it is presumed he will think it still less necessary " to continue in any future year," than before the present attempt was made to shew their superfluity and frivolity, as far at least as regards the errors and omissions attributed to the *Nautical Almanac.*

Additional note, from Schumacher's Journal.

Professor Schumacher, in the 6th Number of his *Astronomical Newspaper*, gives a favourable opinion of the general utility of Mr. Baily's tables, which have been communicated to him by the author; but with respect to the *Nautical Almanac* he observes, (p. 94,) " I understand that the Board of Longitude has within a few years reduced the price of the annual volumes as much as possible, in order that they may the more easily be purchased by seafaring persons, and they would therefore probably not be willing to add to them each year an Astronomical Supplement, which must make the price unavoidably greater. I cannot agree with the author in his comparison of the *Nautical Almanac*, with the *Connaissance des Tems*, with Bode's *Jahrbuch*, with the *Ephemerides of Milan*, and with those of Vienna, which last have been discontinued from the year 1806; and I should doubt whether occultations are particularly appropriate to the determination of the longitude at sea. The seaman requires to know his longitude without delay, and as near as possible to the place of observation; and even if he should happen to be an able computer, which is not very commonly to be expected, the calculation of a lunar occultation might take up so much of his time as to be wholly impracticable while the result would be of any value; and the great advantage of corresponding observations of the same occultation at different places is wholly lost to him."

[No doubt it was from considerations of this nature that Dr. Maskelyne and the Board of Longitude were induced to discontinue the insertion of the greater part of the occultations formerly indicated in the *Nautical Almanac.* It now remains to

be proved whether or no the experiment, which has been made, for the year 1824, will be authorised by any utility that may be found to accrue to the seaman from the elements inserted, when employed in the manner pointed out in the third number of these collections.]

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ii. *Places of the Small Planets for* 1822.

Places of Juno, by Prof. Nicolai, *Bode's Jahrb.* 1824, p. 244.

11 h. 26 m. 8 sec. Greenwich Time.	A. R. in Time.			Declination North.		Log. Dist. from ⊖
1822, Oct. 21.	7h	40m	16s	6°	0′	.2434
Nov. 2.	7	53	9	4	21	.2161
Dec. 4.	8	7	41	0	55	.1434
1823, Jan. 1.	7	54	22	0	44	.1033
8 17,	7	40	13	2	17	.1048
Feb. 2.	7	27	39	4	42	.1277
Mar. 2.	7	21	5	9	16	.2040
Apr. 3	7	40	12	12	50	.3076
7.	7	44	7	13	7	.3200

Places of Vesta, by Prof. Encke, *Bode's Jahrb.* 1824, p. 245.

11 h. 39 m. 30 s. Greenwich Time.	A. R.		Declination South.		Log. Dist. from ⊖
1822, April 1.	266°	50′	17°	8′	.2226
May 1.	271	3	17	14	.1338
June 5.	266	56	18	25	.0619
8 15.	264	26	19	1	.0574
July 5.	259	43	20	20	.0755
Aug. 4.	257	31	22	26	.1504
29.	261	16	24	3	.2267

Places of Ceres, *Bode's Jahrb.* 1822.

Noon at Berlin. 55 m. 29 S. E. of Gr.	Declination.		Passage of Meridian.		
1822, Jan. 1.	26°	1 S′.	0h	19m	P.M.
Feb. 1.	24	54	11	1	A.M.
Mar. 1.	23	12	9	58	
Apr. 1.	21	8	1	50	
May 1.	19	28	7	38	
June 1.	18	54	6	4	
July 1.	20	6	4	13	
Aug. 1.	23	17	2	2	
Sept. 1.	26	30	11	37	P.M.
Oct. 1.	27	8	9	29	
Nov. 1.	25	15	7	34	
Dec. 1.	21	48	5	49	

ART. XVIII. *Results of some Astronomical Observations made in Blackman-Street, during the Months of January and February, 1822, by* James South, F.R.S., *in Latitude* 51° 30′ 3″ N., *Longitude* 21″.8 W.

Eclipses of Jupiter's Satellites.

(Mean Time.)

		h. ′ ″		
Jan. 14,	Emersion of 1st Sat. at	6 23 40.48	with	5 feet Equatorial
		6 23 57.48		30 inch Gregorian
29, ...	2d	6 55 45 30		5 feet Equatorial
Feb. 23, ...	3d	7 8 26.90		5 feet Equatorial

Occultations of Fixed Stars by the Moon.

Star's AR.	Star's Declin.	Mag.	Time of immersion (in Sidereal time.		
Jan. 1.				At ☽'s dark Limb.	With 5 feet Equatorial.
1 12 30 ±	11 40 ± N	7,8	0 21 44.40		30 inch Gregorian.
....	....	....	0 21 44.40		No tremulous motion of the Star, no projection of it on the ☽'s disk; its occultation instantaneous; the observation accurate to one tenth of a second.
Feb. 28,					
4 40 30 ±	26 54 30 ± N	6	7 38 52.67		With 5 feet Equatorial.
A double Star of the 5th class, large red, small blue		6,10	9 36 23.23		Small blue Star } with the 30 in. Gregorian
			9 36 58.73		Large red Star } with the 30 in. Gregorian
			9 36 58.73		ditto with the 5 feet Equatorial.

No tremulous motion of either of these Stars, nor any projection of *either* upon the ☽'s disk, their disappearance instantaneous.

Diameter of Mercury, taken in the direction of his Cusps.

					″
Feb. 14, The planet distant from his greatest elongation	5 days	the mean of	9	measures	= 6.474
15, ...	4 days		10		= 5.912
16, ...	3 days		5		= 6.148

Diameters of Venus taken in the direction of her Cusps.

					″
Feb. 10, The planet distant from her inferior conjunction	27 days	the mean of	10	measures	= 46.224
14, ...	23 days		8		= 48.781
15, ...	22 days		8		= 49.647
28, ...	9 days		10		= 59.656

These measures of Venus, as also those of Mercury, were taken by day-light, and from one to three o'Clock, P. M.

Observations of the Shadows of two of Jupiter's Satellites, seen on the planet's disk in the day-time.

Feb. 14. At 2^h 1′ sidereal time, or about 4^h 26′ mean time, I saw very distinctly the shadow of one of Jupiter's satellites upon his disk. At 2^h 12′ a satellite came off his face, and con-

tinued visible, notwithstanding its proximity to the planet. Measures of the shadow's distance from the satellite were procured. At 2^h $22'$ $10''$ I distinguished another satellite emerging from the same spot whence the preceding had passed off. My attention was directed to the place some minutes previously, in consequence of a luminous point which far exceeded in splendour the faint light of the planet. As yet only one shadow had been observed, but at 2^h $38'$ that of the second became visible; distances of the shadows from their respective satellites were obtained; distances of the satellites from each other were gotten, and also distances of the shadows from each other. The angles of position of the satellites, and of their shadows, were also ascertained. The interior contact of the first shadow with the limb of the planet, was observed at 3^h $20'$; and that of the second at 4^h $4'$ sidereal time. The actual disappearance of the one or the other shadow, owing to the then constant passing of light clouds could not be ascertained. At the commencement of the observations the sun was shining, but the sky, although cloudless, was of a very light blue colour, not at all favourable for sidereal observations. Under more advantageous circumstances in the day-time, I have no doubt measures of the diameters of the shadows might be procured without difficulty.

⁂ The five feet Equatorial has a telescope of five feet focal length, and a double object glass of $3\frac{3}{4}$ inches aperture.

The Gregorian telescope has a speculum of six inches diameter, and thirty inches focus.

The measures of the planet's diameters are taken with a wire micrometer made by Mr. Troughton; its value has been rigorously ascertained, and the uniformity of its screw's thread has most satisfactorily stood the severest examinations.

The declinations of the stars, as also their right ascensions, (here given,) will be found sufficiently accurate to identify them, and are for no other purpose introduced.

Wherever more than one observation of the same phenomenon is given, each may be considered independent of the other; the same clock is used by each observer, but the moment of observation as estimated by each person is noted upon paper before the least mutual communication is allowed; so that any coaxing to make observations tally, is absolutely impossible.

In future communications it is my intention to give all observations in sidereal time, and very much do I wish that other observers would do the same. So would the trouble of converting sidereal to mean time, as also the chance of errors in the reduction be entirely precluded; the considerable incongruities frequently existing between the sun's observed and computed right ascensions would likewise cease to prejudice the observations.

Reference to Plate III.

Observations of the eclipses of Jupiter's satellites are the most unsatisfactory which the practical astronomer is called upon to make; the immersions, however, will afford considerably less incongruity than the emersions; I believe much of this may be attributed to the observer not having his eye well directed to the spot at which the satellite first issues from the shadow, for I have found that since a diagram, of which the annexed is a copy, has been suspended in my observatory, the discordancies have materially diminished. From experience, therefore, I can recommend to others its adoption; the observer will also do well to prefix to the dates of those eclipses which he is likely to witness, the planet's distance from opposition, he will then immediately know which of the diagrams is most applicable to his purpose. The emersion may generally be expected a little to the north or south of the belts.

Art. XIX. *Observations on the Chronometrical Arrangements now carried on at the Royal Observatory, under the authority of the Lords Commissioners of the Admiralty, tending to shew their Inadequacy to the purpose for which they were designed. By* James South, F.R.S.

In offering to the consideration of those who may feel an interest in such matters, the following remarks, I trust, it will be believed that I am actuated by no sinister motive; but conceiving that the acts of public bodies, which have for their object public benefit, when they have had not only notoriety, but even national importance, attached to them, by insertion in the *London Gazette*, become fair subjects for private as well as public inquiry, I have resolved to investigate how far the advertisement relative to chronometers, which appeared in the *London Gazette* of June the 26th, 1821, is likely to prove beneficial to the country.

"*Admiralty Office, June* 25, 1821.

"The Lords Commissioners of the Admiralty, being desirous of increasing the number of chronometers for the use of his Majesty's Navy, and of encouraging the improved manufacture of that important article, do hereby give notice, that a depôt for the reception of chronometers is opened at the Royal Observatory of Greenwich, where the makers will be permitted to deposit their chronometers, in order to their being tried, and ultimately purchased for the use of the navy, or of being disposed of by the proprietors to private purchasers.

"And, for further encouragement, their Lordships will purchase, at the end of each year, the chronometer which shall have kept the best time, at the price of 300*l.*, and the second best at the price of 200*l.*, provided that there have been above ten chronometers in the competition, and that the said best chronometers shall keep their rates within certain limits to be hereafter stated. The other chronometers their Lordships may purchase, as they may think proper, at such sums as may be agreed upon with the makers, and their Lordships have reason to expect, that their an-

nual rate of purchase, for some years to come, will be not less than ten chronometers in each year.

"Every facility will be afforded to the makers, who may place their chronometers in the depôt, for disposing of any of them to private purchasers; and every information will be afforded to purchasers as to the rates of going of the chronometers, of which a strict account will be kept, under the direction of the Astronomer Royal and Board of Longitude.

"The further conditions and regulations connected with this arrangement, may be learned of the Astronomer Royal, at Greenwich, or of the Hydrographer of this office. "J. W. Croker."

"*London Gazette, June* 26, 1821."

In consequence of this notification, it appears that upwards of thirty chronometers have been sent by their respective makers, (or perhaps proprietors would be more appropriate) to Greenwich, and the printed paper, which now lies before me, is entitled "An Account of the Rate of the several Chronometers on trial at the Royal Observatory, for February, 1822." Now, I must acknowledge that, from the style in which the advertisement was couched, I augured that little good would result from it: hoping, however, that my forebodings might prove unfounded, I was willing to wait till something like the *modus operandi* should be developed, and, I am sorry to say, now that such knowledge is acquired, the plan seems so inefficient, that I cannot but regard it as altogether futile. Whether, however, I may support my opinion upon solid grounds remains to be determined.

It must not, however, be supposed, from any thing that may escape me during the discussion, that I am disposed lightly to value the importance of good chronometers; far otherwise, no one estimates it higher, and, considering as I do, that the lives of British seamen are oftentimes dependant on their good or ill performance, I would withhold neither pains, labour, or expense, to procure them as perfect as the head and hands of man could make them. Whilst, however, condemning the mode adopted, I cannot allow this opportunity to escape, without commending, in the highest manner, the principle which gave it birth. Joining, therefore, most heartily, in the object to be gained, we shall only differ as to the means of effecting it.

The essentials of a good chronometer are these; that its rate should be uniform; undisturbed to any extent by alterations either of position or of temperature. A chronometer which answers these conditions may be considered fit for nautical, or (when a clock cannot be procured) for general astronomical purposes.

If, then, such be the attributes of a good chronometer, it may be right to inquire what are the means which should be employed to determine, with the greatest certainty, whether a chronometer does or does not possess them. Having settled

this matter, we shall see how far the trials to which the chronometers now in the depôt are submitted, accord with these; and we shall then be enabled to draw such inferences as the comparison may warrant.

Now, the rate of a chronometer is usually deduced from daily comparisons made with a good clock adjusted commonly to mean time; I would, however, prefer immediate comparisons with the transit clock, that being always considered the best in an observatory, and unless there are substantial reasons to the contrary, I would unquestionably have the chronometer adjusted to sidereal time, by which all trouble of reduction will be spared, and the errors arising from the unsatisfactory nature of solar transits entirely precluded.

Should, however, necessity oblige us to have our chronometer shew mean time, there is one circumstance which we may avail ourselves of in comparing it with the sidereal clock. The acceleration of sidereal on mean time is such, that the former gains on the latter one second in about six minutes, now the box chronometer usually beats half seconds, and consequently synchronises with the clock's pendulum twice in this time; these instants are, with a little attention on the part of the comparer, so easily ascertained, that, to make an error in the comparison equivalent to one twentieth, or even one fiftieth part of a second would be almost unpardonable. When pocket chronometers are compared, the same principle may be acted upon. These generally beat five times in two seconds, but, owing to the seconds' circle being ill divided, and the seconds' hand not accurately centred, equal accuracy cannot be expected.

The instrument, whether large or small, is always included in a box, whose base and top are connected together by four sides, which we will denominate A, B, C, D. As sent home by the maker, it will be unclamped and horizontal; in this state, therefore, it should for a few days be compared with the clock, and if its rate seem uniform, let it now, clamped in its gimbols, be placed vertical, by making the box rest upon the side A; on the day following move it one quadrant, by placing the box upon the side B; on the next day, vary its position another quadrant, by resting its box upon the side C; and on the subsequent day let it be passed through the remaining quadrant, by making its box rest upon the side D: it may then be restored to its horizontal position, and if, on repeating these experiments, examination should detect no material difference in the daily rates, a great point will be gained. During these observations the temperature should be kept as equable as possible; the thermometer, therefore, should be frequently appealed to, otherwise the inferences deduced will be liable to some suspicion.

Having gone thus far, it will be prudent to unclamp it, and

try whether a slight motion given to it will be productive of any alteration in its rate; and for this purpose it may advantageously be carried quickly up and down stairs frequently during the day; may be placed in a carriage driven quickly through the paved streets; or, should this be inconvenient, it may be slung over a servant's shoulders, who shall ride with it for a few miles on a hard-trotting horse; if proof against these trials, it may be clamped, and the same experiments repeated. Should no material alteration of its rate be elicited by these various contrivances, it may be said to have answered the first condition.

Let it now be placed for twenty-four hours in a temperature such as may at any time be obtained, say 50° of Fahr., its rate well determined, and on the next day let the temperature of the room be raised to 80°, and the day following to 110°; now, if on examination of the daily rates it should seem a matter of indifference whether the temperature is 50, 80, or 110, it will only remain to put it to the most severe test of any, namely, that of exposing it to temperatures below 32 on one day, and from 110 to 120 the next day: in the ordinary winter seasons, the former is not difficult; but, when natural cold cannot be had, artificial may; for frigorific mixtures are not expensive: and, as to the latter it is always within our reach *. To what extent cold may be safely employed I do not know, it is a subject upon which it was my intention to have made some experiments during the last winter, had it not proved too mild for the purpose. There seems, however, to be doubts as to what degree of cold the oil will usually bear without losing its fluidity, as also what diminution of temperature the spring will endure, without having its elasticity impaired. Perhaps also, under instantaneous exposures to intense cold, a slight deposition of water upon the spring might occur, which, if it lead to the oxidation of the metal would unquestionably injure its power. From experience, however, I well know that a temperature of 20° on one day may be succeeded by one of 110° on the next day, with perfect safety to chronometrical economy; and, I also know, that extremes such as these, are trials that few, very few chronometers will bear, without having their rates materially deranged †. A chronometer therefore satisfying these conditions, as also the former, may be indeed pronounced good, and is fit for nautical, or when a clock cannot be procured for general astronomical purposes.

* For this purpose a small stove placed in the centre of the room in which the inquiries are conducted, will be found very useful.

† One day is named as the period allotted for each alteration of position, *&c.* The experimenter, however, will do well, should time allow to continue his chronometer under each variety of circumstance, two, three, or four days.

But it may be asked, is it not to be expected that a chronometer which shall in one position keep a better rate than twenty others, will retain its superiority under every other circumstance? To this I unhesitatingly reply, no. A chronometer may go well whilst horizontal, and ill whilst vertical; and *vice versâ*, or it may go well whilst in a state of quiescence, and ill when put in motion; it may go well when placed in an atmosphere which is temperate, and ill when exposed to extremes of heat or cold.

An instance occurred to me three years ago in a box chronometer, made purposely for me by one of our first artists, which, while it remained horizontal, kept its rate remarkably well; but when placed vertical its rate was so much altered, that further trials of it till it had been returned to the maker were altogether useless. Again, the same chronometer was incapable of sustaining considerable alterations of temperature, although slight differences produced no sensible effect: again, on its return to me, the motion of a carriage occasioned some deviation from its rate, an evil which its maker afterwards completely remedied. But in these instances just alluded to, the defects were removed; in another instance, however, the attempts have not been so successful; in the early part of last year a gold pocket chronometer was made for me by the same person, and although, whilst horizontal and quiescent, it would even under severe alterations of temperature dispute the prize with most, still would not bear alteration of position without altering its rate; it has on this account been twice or thrice returned to the maker for correction; but, notwithstanding all his pains and labour, he has not succeeded, for he informed me two or three days ago that it was still imperfect; and he is apprehensive that, to enable it to satisfy the only condition wanting, (for it is in every other respect an invaluable instrument), will probably for ever baffle his endeavours.

Again, during the last spring two pocket chronometers were placed under my care, by Major-General Sir Thomas Brisbane, K.C.B. The one of them was of gold, the other of silver; the former was an old favourite, the latter was deemed of no value, and considered scarcely to merit the name of a chronometer; they were included in their respective boxes, and their rates ascertained daily by comparisons with my transit clock; the gold one kept on the same second for a fortnight or three weeks, but the silver one did not; at this time being occasionally employed in inquiries into the differences of longitude of my observatory and that of Greenwich, the gold one amongst some others was deemed suitable for the purpose, its error as also those of its co-travellers were well ascertained before they were dispatched hence, and immediately on their return home comparisons were again made; on con-

trasting the errors which they now had with those which respectively belonged to them previous to their being sent away, (the proportional rates for the interval having been applied to them,) this gold chronometer which had kept a more uniform rate than any whilst quiescent, was so much affected by the journey, that a deviation from its rate amounting to more than a second was the result, whilst the others may be considered as having suffered no sensible inconvenience, for a tenth of a second was the maximum of alteration observed in either. Finding this occurrence where it was least likely to have been anticipated, I suspected that perhaps the error might have been my own, originating in inaccurate comparison; to put this matter to the test, all the same chronometers were transmitted, as also was the silver one on the day following, and the same precautions being taken prior to their dispatch, and subsequent to their return, the same results relative to the gold one were obtained; whilst the silver one, when the proportional rate for the intervening time had been applied, gave precisely the observed error. This was no accidental circumstance, the experiments, somewhat modified, were several times repeated, and uniformly with similar results. Observing the valuable property possessed by the silver one, I returned it to its maker, requesting him to lessen its rate, which was cumbersome; this being effected, I frequently carried it about in my pocket, together with the gold one, and whilst I could place no reliance upon the latter, the former scarcely ever led me into an error of the time exceeding one or two tenths of a second, notwithstanding it sometimes met with rather rough usage; indeed, this almost despised chronometer was one of the best I ever saw. That it was difference of position which occasioned the error found in the gold one, was inferred from a knowledge that it would bear very well considerable alterations of temperature. A chronometer, intended for the pocket, should be rigorously indeed examined, as to the effect produced upon it by alterations of position, seeing that the mere reclining of its wearer on a couch, may lead to considerable alterations of rate; indeed, from many opportunities of judging, I am inclined to believe, that it is more difficult to procure a good pocket, than a good box, chronometer.

I have here only alluded to some of the instances which have occurred under my own observation, where chronometers, good in one respect were bad in another. Others might have been adduced, but those already mentioned, will, I hope, suffice. They are assertions, it is true, the particulars of which are not brought forward; but the like are so frequently occurring, that he, indeed, can have had no experience who has not found them practically correct. To make a chronometer keep a good rate, when exposed to no change of position except what it must ne-

cessarily undergo in the act of winding, or to such change of temperature as it may usually meet with in a drawing-room, is neither hard nor difficult; but to enable it to sustain it in alterations of position, as well as of temperature, such as I have hinted at, is no easy task. These latter qualities depend upon the accuracy of its adjustment, and perhaps I might add, upon the uniformity of the materials of which the spring and balance are constructed, desiderata not to be arrived at without considerable sacrifices of time and labour. It is these, in fact, which so much enhance its value. But lest it should be supposed that I am giving to these adjustments more importance than they really merit, I shall only say, that two of our first-rate makers have sold as common silver watches, for the trifling sum of twenty-five pounds, what were in every respect equally well constructed with their best chronometers, and which wanted only the labours of adjusting to render them such; nor was the plan abandoned, till it was found that these watches occasionally got into the hands of persons for whom they were not made, who, ignorant of the terms upon which they had originally been sold, were sometimes enabled to bring their maker's name into disrepute.

But it may perhaps be urged, that the plan of experimenting which I have here proposed, although very well in theory, is too difficult for practice. To this I can only reply, that a box chronometer is now before me, which has, *propriâ personâ*, undergone, and *satisfactorily* undergone, the *very* trials here alluded to: and, what is more important for the Lords Commissioners of the Admiralty to be informed of, *is*, that for a chronometer such as this, I gave to its good and honest maker * the sum of fifty guineas.

If, however, their Lordships should deem it right, *still* to have their chronometrical affairs conducted at the Royal Observatory, (and as to the propriety of this, among scientific men, there seems some doubt), I will indulge a hope, and a sincere one it is, that if 500*l.* *must* be annually given for two chronometers, that two may be procured, at least as *good*, as any private individual may get for ONE.

Whether, also, it was necessary to decoy chronometers to Greenwich, by promising 500*l.* for what is worth but ONE, will perhaps admit of doubt. I cannot, however, but think, that a sufficient emulation would have been excited amongst the real makers, (and in this instance none others are worthy of consideration,) had the Lords of the Admiralty engaged annually to have published in the *London Gazette* the *name* and *residence* of that artist whose work had been declared the best, whilst the 400*l.*, thus annually saved, might be appropriated to some

* Mr. Molyneux.

scientific purpose, (for I would protect it from Humean fangs,) and perhaps to none better than restoring to its *pristine* excellence the *Nautical Almanac*, or *Astronomical Ephemeris.*

But, to return from this digression; by reference to the preceding pages it will be seen what are the requisites of a good chronometer, and what are the means which, *if* adopted, seem most likely to enable us to select the good, and reject the bad. Having also shewn that a chronometer may be good in one respect, yet bad in another, it remains that we should see how far the mode of trial sanctioned by the Admiralty, and carried on under its authority, accords with that which we have considered indispensable.

The Admiralty places its chronometer horizontal, and daily comparisons with a good clock are enforced: our plan also requires this. The Admiralty exposes its chronometers to alterations of temperature, such as are to be met with in a common sitting-room*: our plan does the same. We try our chronometer vertically, and in different directions of its axis; the Admiralty does no such thing. We try the effect of motion upon our chronometer; the Admiralty does no such thing. We expose ours to extremes of cold; the Admiralty does no such thing. We try ours in extremes of heat; the Admiralty does no such thing. We compare the various results produced by these alterations of circumstances upon our chronometer, because we wish to take not *any thing* for granted; the Admiralty having ascertained one fact, and that, perhaps, of the least importance, takes *every thing else* for granted. We *take* all this trouble, that we may get a good chronometer; the Admiralty *saves* all this trouble, and will probably get a BAD ONE.

On comparison, therefore, of the two modes, knowing, as I do, and shewing, as I trust I have done, that the various trials I have advised are necessary, the only inference I can draw is, that the chronometrical arrangements at present conducted under the sanction, and by the authority, of the Lords Commissioners of the Admiralty, are INADEQUATE TO THE PURPOSES FOR WHICH THEY WERE DESIGNED.

J. SOUTH.

Blackman-street, March 22, 1822.

* The printed papers inform us, that the chronometers have been exposed to differences of temperature equal to 8° of Fahrenheit.

Art. XX. *Miscellaneous Intelligence.*

Mechanical Science.

§ Optics, Agriculture, the Arts.

1. *Extract of a Letter from Professor Oersted, of Copenhagen.*

I can at present only give you the result of an investigation, which is so far void of interest as it only relates to the confirmation of an earlier discovery, I mean that of Zamboni, concerning the electricity produced by the contact of a single solid conductor with a single fluid conductor. You know that such experiments are extremely delicate; they seem to have been repeated only by *Mr. Erman*, and this celebrated and ingenious philosopher complains much of the irregularities which the experiments present. I have found in the electro-magnetic multiplier, invented by Schweigger, a mode of making these experiments with the greatest ease. I take two plates of zinc, of different breadths, one, for example, 3 lines broad, the other $1\frac{1}{2}$; I place them in a diluted acid, and I make a communication between each of them and one of the extremities of the metallic wire of the multiplier. The action is thus rendered very sensible. The wider plate assumes in this galvanic arc the place of the copper, the narrower that of the zinc. When we take two plates which are perfectly equal, and attach them to the extremities of the multiplying wire, we obtain no effect, if we plunge both plates at once into the liquid; but if we immerse one before the other, that which has come the last into contact acts as a less oxydable metal.

Heights of Barometer, 25th Dec. 1821.

Hanover. by Luthmer.	E.I.	Th. °F.	Altona, by Schumacher.	E.I.	Th. ° F.
5 A.M.	28.42	41	Dec. 24, 19^h 0^m	28.39	42
8 ,,	28.35	44	,, 20 33	28.38	42
1 P.M.	28.34	49	,, 25, 0 14	28.32	45
7 ,,	28.44	43	,, 1 50	28.32	46
10 ,,	28.50	43	,, 2 38	28.31	46

Schumacher's Astr. Nachr., No. 6.

2. *Remarkable Dichroism of Tourmaline.*—A very interesting specimen of dichroitic tourmaline, in the cabinet of Mr. Allan, exhibits the most singular contrast of colours ever yet found in any substance. The plate is cut perpendicular to the axis of double refraction, and also to the axis of the prism. In the direction of the axis the colour is a deep and brilliant blue, while in a direction at right angles to the axis the colour is a very pale red, approaching to pink.—*Edin. Phil. Jour.* vi. 177.

3. *Orthometer and Pleometer, instruments of Navigation.*—Mr. Perkins has applied the mercurial level in an ingenious manner to the construction of an instrument, calculated to facilitate the sailing of ships. A horizontal tube is turned up vertically at each end, to the height of about three inches. It is then filled with mercury, so that the metal rises about an inch in the two legs, to each of which a float is fixed, forming one end of a lever, as the index does the other end, which is so adjusted that the two indexes are in the same horizontal line, when the mercury is level on the two legs; but when the mercury is unequal, then the indexes are one higher and the other lower than the horizontal line. Two instruments of this kind being fixed against the sides of a ship's cabin, one parallel to the keel (called the orthometer,) and the other at right angles to it, (the pleometer,) will shew the angular changes in the position of the ship, occasioned either by the distribution of the cargo, or the impulse of the wind.

The instrument is suspended by two points, one fixed and the other an adjusting screw; and, that the mercury may not be thrown about by sudden changes of position, a stop-cock is attached to the middle of the horizontal tube, by which its bore or capacity may be diminished in any proportion, and the instrument made to exhibit the average inclination of the vessel, without derangement by sudden heaves.

When the vessel is at sea, and sailing to most advantage, the adjusting screw is turned till the indexes are in the same line, and this adjustment will ever afterwards indicate the trim of the vessel, as long as no material change takes place in the quantity or disposition of the cargo.—*Trans. Soc. Arts.* xxxix. 127.

4. *Arithmometer.*—A French artist, M. Thomas, of Colmar, honorary director of the Phœnix Company, has obtained a brevet of invention (patent,) for a machine of calculation, to be called the arithmometer. It has been presented to the Society for the encouragement of National Industry, and by it a person unacquainted with figures may be made to perform, with wonderful promptitude, all the rules of arithmetic. The most complicated calculations are done as readily and exactly as the most simple; sums in multiplication and division of seven or eight figures require no more time than those of two or three.

5. *English Buhr Stones.*—Messrs. Bishop and Co., of Nant-y-Moch, near Holywell, in Flintshire, have, within the last two or three years, introduced millstones into use, of English origin, which are equal, and in some cases apparently superior, to the celebrated French buhr. Till this source of millstones was discovered, none could be found that could be substituted

for the French buhr, especially in the grinding of wheat, for they alone possessed the required degrees of hardness, toughness, and that cellular structure which is necessary to perform the operation rapidly, without causing the mixture of earthy matter with the flour, or without heating it.

The French buhr occurs in detached masses, or blocks, in the fresh-water limestone formation of the Seine. It is intermediate between hornstone and calcedony. No similar stone has been found in the analogous formation of the Isle of Wight, or any where else in this country, but the stone which Mr. Bishop has successfully substituted for it occurs near the middle of the eastern ridge of the Halkin mountain, which itself is composed of limestone rocks. The buhrstone, or entrochital hornstone, appears as a bed about four yards thick, dipping easterly, like all the other strata of the mountain. It sometimes varies in quality, rotten masses occurring, and also portions that are too close for the miller. Still the corallite structure pervades the whole, the entrochites being perfect and entire in some instances, while in the chief part of the bed the casts alone remain, thus leaving the rock vesicular; and in this respect differing from the nature of the pores in the French buhr, which appear to have been caused by erosion, their edges being rusty and impure, whereas those in the Halkin buhrs are of pure flint, and exceedingly sharp and hard.

These stones have now been tried for the last three years in many mills, and evidence of their excellence and superiority in all respects, of the most satifactory kind, has been obtained. Hence there is no doubt they will get into general use, and the more they are made known the better.—*Trans. Soc. Arts.* xxxix. 55.

6. *Oil for Watch-work, &c.*—Oil used for diminishing friction in delicate machinery, should be free from all acid and mucilage. The following is the process: (M. Chevreul's) recommended as the most convenient for procuring it in the most favourable state. Put into a matrass or glass flask a portion of any fine oil, with seven or eight times its weight of alcohol, and heat the mixture almost to boiling, decant the clear upper stratum of fluid and suffer it to cool; a solid portion of fatty matter separates which is to be removed, and then the alcoholic solution evaporated in a retort or basin until reduced to one-fifth its bulk. The elaine or fluid part of the oil will be deposited. It should be colourless and tasteless, almost free from smell, without action on infusion of litmus, having the consistence of white olive-oil, and not easily congealable.

7. *On the setting of cutting Instruments.*—Mr. Reveley recommends the use of soap instead of oil for the setting of

razors and other cutting instruments on a hone. His own words are: "Not having any oil to set my razor, it occurred to me to try the soap I was washing with, called palm-soap, and I found it so completely to answer my purpose, that I have constantly used it ever since instead of oil, both for razors and penknives. It sets quicker, gives a good edge, and removes notches with great facility: it is a more cleanly material, oil being liable to drop on, and soil any thing it comes in contact with; dust will frequently get into oil which will spoil the edge, and in such case it must be changed. It is as cheap or cheaper than oil, a small square of palm-soap costing only threepence, which will last for a great length of time. The operation is performed as follows: having first cleaned your hone with a spunge, soap, and water, wipe it dry: then dip the soap in some clean soft water, and wetting also the hone, rub the square of soap lightly over it until the surface is thinly covered all over, then proceed to set in the usual way, keeping the soap sufficiently moist, and adding from time to time a little more soap and water if it should be necessary. Observe the soap is clean and free from dust before you rub it on the hone; if it should not be so, it is easily washed clean; strop the razor after setting, and also again when you put it by, and sponge the hone when you have done with it."

Many instrument-makers and others have tried Mr. Reveley's method of setting instruments, and speak in high commendation of it. Among them are Messrs. West, Pepys, Long, and Frewen.—*Trans. Soc. Art.* xxxix. 137.

8. *Improved Drawing Tablets.*—The extra stout drawing-boards, or card-boards as they are usually called, are liable to many defects. They are made by pasting several sheets of paper together in the manner of common pasteboard, and are then pressed and rolled. In consequence of this process, it frequently happens that a small portion of paste is left on the surface, which, escaping observation at first, causes injury when a drawing is made on the board. In drawing also if frequent re-wetting on a particular part be necessary, the paste sometimes gives way, the sheets separate, and blisters rise. It has been remarked too, that drawings made on such boards, and hung up in rooms where fires are seldom made, spoil from the tendency of the paste to mould or mildew. Another defect is, a hollow or spongy texture, arising from the indiscriminate mixture of linen and cotton fibres in the pulp of the paper; and another, the deleterious effect of bleaching by oxymuriate of lime, a process which, though it makes the paper beautiful in appearance, leaves a small portion of muriatic acid, which speedily destroys the fine and delicate tints laid upon it.

In consequence of these defects, Mr. Steart, of the Montalt

paper-mills near Bath, has submitted to the Society of Arts specimens of drawing-boards, which he calls Lino-Stereo tablets, entirely free from these defects, and which have been found to answer every trial made of them. The only material used in their manufacture is the best and purest white linen rags, all muslins, calicoes, and every other cotton article being rejected; and, they are not composed of several sheets, but are moulded from the pulp of any required thickness of one entire mass, so that no separation of their parts can take place, either by wetting or mouldiness. These tablets are made with either a rough or smooth face, the former for drawings made with pencil, chalk, or crayon, the latter for paintings in water-colours, or other delicate works. They are also tinted of various shades of drab-gray, sand-colour, *&c.*, the pulp being dyed in the vat before it is made into tablets.—*Trans. Soc. Art.* xxxix. 43.

9. *New Green Pigment.*—M. Bizio of Venice describes a new pigment, obtained by boiling a hectogramme (1544 grains) of coffee-powder in water, reducing the infusion by evaporation to eight hectogramme (28 oz. 4 dr.), adding an equal weight of sulphate of copper dissolved in water, and precipitating by solution of caustic soda. The deposit found weighed 105 grammes (1622 gr.); when dry, it was a fine green colour, and the more exposed to air whilst moist, the brighter it became. Water, ether, alcohol, and alkaline subcarbonates, had no effect upon it. Ammonia and potassa acted upon it: soda did not alter it. It resists acids sufficiently well, and, with the exception of the sulphuric and oxalic, no others destroy the colour totally. They, however, dissolve it, and it is mentioned that acetic acid produces a solution with it of a very fine green colour.

10. *Economical Matting.*—Mr. Salisbury, with the intention of giving employment to such as were destitute of it, engaged a number of the paupers of the parish of St. George's, Hanover-square, to collect a quantity of the typha latifolia (flag or greater cat's tail), and to manufacture it into mats, baskets, hassocks, chair bottoms, *&c.*, with the intent of substituting it for the scirpus lacustris, or rush, which, though it grows abundantly in some places, is by no means in sufficient quantity to supply the demand made for it, so that large importations are made from Holland. On a close comparison of the things made with it, with those made with Dutch rushes, the work appeared capable of being made equally neat with either material, and an examination of two pieces of matting, which had lain side by side, and had their places changed occasionally, indicated that they would wear equally.

Hence the typha will probably afford a material of great use

to poor people, and as it abounds in all marsh ditches and uncultivated swampy ground, an abundant supply may be obtained by every one.—*Trans. Soc. Arts.* xxxix. 52.

11. *On the Preservation of Manure.*—The following is part of a letter from M. de Fellenberg, of Hofwyl, to the editors of the *Bibliotheque Universel*, on the important point of preserving manure, with the least injury to it, or loss, until required to be put on the land.

" Those who are most careful whom I have known in Switzerland, obtain an advantageous result by spreading the dung of the stables very regularly in thin layers on the heap, and pressing each down by frequent treading, so as to make a close and compact mass, which is surrounded by a kind of twist made with the tubes of litter, of which the ends are turned towards the interior of the heap, so as to retain the fluid parts which might otherwise escape. These persons, satisfied with their process, inasmuch as it prevents all mouldiness and decomposition of their manure, do not seek after the cause of their success, nor after any improvement which the instructed agriculturist may desire. By making the heap of manure very dense, and by excluding in that manner the influence of air, one may completely prevent fermentation and decomposition; but in pressing the heap, the tubes of straw, which ought to become filled and impregnated with the fluid parts of the manure, are flattened and broken. I gain the same advantage by making my heaps of dung so, that they shall be compact and without vacuities, but without pressing them. I moisten them abundantly with fluid manure, as soon as they are began to be formed; and this moistening, or watering, is repeated each time that from the elevation of vapours from the heap, it is judged that fermentation is commenced. By means of this process, the fermentation is continually checked. Whatever might have evaporated from the dung, is returned to the interior by the fluid thrown on the surface, which fluid is properly the essence of the dung; the tubes of straw, which served as litter, are impregnated, and this fluid forms certain chemical combinations, which enrich the mass both in its quality and quantity." —*Bib. Univ.* 1822, Jan. (23.)

II. Chemical Science.

1. *Process for preparing Lithia, by M. Arfwedson.*—M. Arfwedson describes the following as a more convenient process than that hitherto known for the preparation of lithia. It consists in exposing an intimate mixture of triphane or spodumene in a fine powder, with quick lime, in a Hessian crucible, to a

very strong heat. The heated mass is to be dissolved in muriatic acid, and the solution evaporated to dryness, in order to separate the silica. Sulphuric acid is afterwards added, and the mass is heated till the greater part of the muriatic acid is driven off. The residue is next diluted with water, and the liquid separated from the gypsum by strong expression. The solution obtained is then digested with carbonate of lime, in order to precipitate the alumine, and afterwards filtered and evaporated. The crystals of sulphate of lithia are then easily separated from the remaining sulphate of lime. If it is desired to prepare the lithia in the state of carbonate, the sulphate is to be decomposed by acetate of barytes, or of lead, and the acetate of lithia obtained decomposed by heat, and the carbonate separated by solution, *&c.*—*Edin. Phil. Journ.* vi. p. 12.

2. *Preparation of Ethiop's Mineral.*—Dr. Taddei recommends the following process for the preparation of this substance, as being one which effects the combination immediately, and in a more perfect manner than that generally employed. Put one part of sulphuret of potash into a mortar, with three or four parts of running mercury; triturate together, adding a little water by degrees, until the whole is reduced to a homogeneous black paste; then add flowers of sulphur, in equal quantity to the mercury employed, and mix the whole by a short trituration. Then wash the whole; and filter with repeated portions of water, till all the alkaline sulphuret is removed.

Ethiops thus prepared is not of the black colour of that obtained by simple trituration, but it is a more perfect combination. Dr. Taddei says, that the addition of a little sulphuret of potassa to the mixture of sulphur and mercury, does not do away with a long trituration, but that, proceeding as above, the substance is prepared instantly.—*Giornale di Fisica*, iv. 12.

3. *Preparation of Arseniuretted Hydrogen.*—M. Serrulas, in his highly interesting Memoirs on the Alloys of Potassium, gives the following process for the preparation of arseniuretted hydrogen. It depends on the action of water on the triple alloy of arsenic potassium and antimony. For this purpose a mixture must be made of two parts of antimony, two parts of cream of tartar, and one part of oxide of arsenic, they must be well triturated together in a mortar, and heated strongly for two hours in a close crucible. The alloy which results when in contact with water, produces hydrogen gas saturated with arsenic, and it may be preserved for any length of time in closed vessels. For an experiment or lecture, 8 or 10 grammes, (123 to 154 grains,) reduced to a coarse powder, are to be

thrown quickly under a jar filled with water, and inverted in a glass basin, also containing water, several decilitres, (6.1 c. i. each.) of arseniuretted hydrogen will be obtained in two or three minutes.

In this process there is no want of bottle, tube, acid or fire, and the facility it affords of operating at common or low temperatures, is probably very favourable to the complete saturation of the hydrogen by the arsenic.—*Journ. de Phys.*, xciii. 135.

4. *Oxide of Titanium.*—Mr. Rose of Stockholm, has attempted to analyze oxide of titanium, by converting it into a sulphuret. The method adopted was to heat the oxide highly in a porcelain tube, and pass sulphuret of carbon in vapour over it. The sulphuret thus obtained was a greyish-yellow mass, bordering on green, which, by the slightest touch took a metallic lustre, resembling the magnetic sulphuret of iron. The oxide of titanium is considered as containing 33.93 hundredths of its weight of oxygen. It does not possess any of the characters of a salifiable base. The nitrate and muriate, as usually described, are salts, with bases of potash or soda. The oxide combines with water, reddens blues, and drives off carbonic acid from carbonates.—*Edinburgh Philosophical Journal*, vi. p. 15.

5. *Cadmium in Metallic Zinc.*—Dr. Clarke has discovered the presence of cadmium in the metallic zinc of commerce. Its quantity is very small.

6. *Solution of oxide of Copper in Ammonia.*—M. Berzelius, in his description of a method of analyzing the ores of nickel, says, that he had not been able to determine whether oxide of copper is soluble in ammonia or not. " It is certain that all those solutions, which are generally regarded as oxide of copper in ammonia, are double salts, with excess of base. I digested oxide of copper in concentrated ammonia for eight days in a stopped bottle. The solution became of a light-blue colour in 48 hours, and it did not afterwards increase. A drop of *carbonate of ammonia* let fall into the liquor, immediately dissolved a part of the oxide, and made the lower stratum of the liquid of a deep blue colour."

When an ammoniacal solution of oxide of copper is mixed with caustic potash, the oxide of copper is precipitated in a few seconds; and if the quantity of potash is sufficient, it is entirely deposited in the form of a blue hydrate, which it is very easy to wash. When well washed it yielded blue hydrate of copper, combined with two atoms of water; it does not retain any trace of potash.—*Annales de Chimie*, XVII.

7. *Precipitation of Nitrate of Silver by Chlorine.*—In the Annales de Chimie, tom. xviii. p. 270., it is said, in a note, that a gas spoken of in the text as chlorine, could not be chlorine alone, but must have contained muriatic acid, inasmuch as pure chlorine cannot precipitate solution of nitrate of silver. This extraordinary statement would not require notice, but that it professes to be made by the editor of that work, whose authority, in such a case, is so great, that it would almost lead one to suppose the discovery of some new property in that substance. The fact, however, that pure chlorine can precipitate nitrate of silver, and even act on the dry salt, is so well and easily established, that one must suppose the note to have originated either with a careless transcriber or translator, notwithstanding the R. attached to it.

8. *Analysis of Alum, and number of Alumina.*—Dr. Thomson has given an account of his analysis of alum, made with the view of ascertaining accurately the number of alumina. The composition of alum is given as follows:

4 atoms	sulphuric acid . .	20.	or	32.8542
3 ——	alumina	6.75	—	11.0882
1 ——	potash	6.	—	9.8562
25 ——	water	28.125	—	46.2012
		60.875		99.9998

Or otherwise,

3 atoms	sulphate of alumina	21.75	or	35.72885
1 ——	sulphate of potassa	11.00	—	18.06975
25 ——	water	28.125	—	46.20123
		60.875		99.99983

Dr. Thomson denies that alum contains any bisulphate of potash. It has been said that when solutions of sulphate of alumina and sulphate of potash are mixed together, a precipitate falls consisting of alumina, the acid having been taken from it to convert a portion of sulphate of potash into bisulphate. On repeating the experiment with pure solutions, Dr. Thomson could obtain no immediate precipitate; but after 24 hours, crystals of alum were deposited. There is, therefore, no evidence of a bisulphate of potash in alum, and the analytical experiments are quite against any such supposition.—*Annals of Philosophy*, iii. 168.

9. *Gas from Coal Tar.*—It has been found, by experiment, that the coal-tar liquor, which is sometimes considered as waste by those who make gas, if mixed with dry saw-dust, exhausted logwood, or fustic, to the consistence of paste, and allowed to remain till the water has drained off two cwt. of the

mass being put into the retort instead of coal will produce more gas, and be less offensive than the same quantity of canal coal. This process will probably be found very convenient in some circumstances for the consumption of the tar produced by the distillation of coal in gas-works.

10. *On the formation of Ice in the beds of Rivers.*—Dr. M'Keever of Dublin, whilst reasoning on this particular formation of ice, which has never yet been well explained, suggests, that it may be occasioned by radiation just in the way that hoar-frost is formed. It is mentioned from the observations of Mr. Garnet, in a former volume of this Journal, that such ice is seldom seen adhering to any substance except rock, stones, or gravel; that it is always found in greatest abundance in proportion to the magnitude and number of the stones composing the bed of the river, combined with the velocity of the current; and that it does not occur where mud or clay is deposited. Dr. M'Keever thinks it probable, that in consequence of the greater radiating powers, as regards heat, of these substances on which the ice is deposited, an effect of depression of temperature may be produced by the heat they throw off into space, sufficient, when it takes place in water nearly at the freezing point, to decide the formation of ice at their surface, and the rapidity of the current of cold water, he thinks, may be active in reducing the temperature of the bed of the river more rapidly, as well as in removing the heat evolved by the congelation of the successive portions of water. The great objection to this theory, it is remarked, is Professor Leslie's statement, that no radiation takes place in water; but it is justly observed, that from the evident difficulty of making the experiment accurately, that point is not yet decided.—*Ann. Phil.* iii. 187.

11. *Singular Congelation of Water.*—M. Pictet relates, that whilst engaged in examining some subterranean excavations in a bed of lava, near Niedermendig, he had occasion to observe a fact that had previously drawn his attention in many natural caverns in the midst of summer, and of which he had not yet found an explanation. "I observed in some places water falling drop by drop from the roof on to the floor, or against the sides of the cavern. Wherever this happened, there was beneath, a mass of ice of a certain thickness; nevertheless the temperature of the air was 3.5 R (39.8° F.), and I believe that at no time did it ever descend to zero in these subterraneous places.—*Geneva Mémoires* i. p. 151.

12. *On the heat of solutions crystallized by exposure to air.*—When a hot saturated solution of Glauber's salt is suffered to cool in a closed flask or phial, it remains fluid until the stopper

is removed, when it immediately crystallizes, and its temperature becomes elevated. Dr. Thomson has lately experimented on this phenomenon; not with a view of ascertaining the cause of the crystallization, but on the source of the heat evolved. Sulphate of soda and carbonate of soda were the salts used; 51 parts of crystallized sulphate of soda to 49 parts of water, make a good solution for the experiment. If carbonate of soda be used, 1 part should be dissolved in 4.22 parts of water, and the solution suffered to cool below 50° F. When the sulphate of soda is used, the crystallization of the mass is certain, at a temperature under 50°, on removing the stopper, and the rise of temperature equals 24° F. If the carbonate be used immediately the cork is drawn, an abundant precipitation of small crystals in stars takes place, and the temperature rises 14° F. From estimation made of the rise of temperature; the whole quantity of matter heated; the quantity of salt solidified; of water of crystallization combined with it, *&c. &c.*, Dr. Thomson concludes, that the latent heat of the water of crystallization was, in both cases, the sole source of the increase of temperature observed.—*Annal. Phil.* iii. 169.

13. *Heat in the moon's rays.*—Mr. Howard, by exposing a concave mirror to the moon's rays, appears to have observed a slight elevation of temperature in the focus, the thermoscope used being particularly delicate. The experiment, on being repeated by M. Pictet, gave exactly opposite results; for in place of an elevation of temperature, there was depression, and the thermoscope sank, although the same instrument that was used by Mr. Howard. M. Prevost, in remarking on these results, points out some very important precautions to be taken in this and similar experiments. As in a clear night, a concave mirror exposed to the heavens will almost always indicate a depression of temperature in its focus, in consequence of its intercepting the warmer rays from the earth, and substituting those which are less heating from the heavens; the quantity of effect of this kind becomes an important correction in estimating the heat caused by the lunar rays, when concentrated by a concave reflector. The effect is variable, being very much diminished by opaque vapour, and by reflection from such terrestrial bodies as may enter into the field of the mirror: in winter also the difference between the temperature of the earth and the sky is diminished, and consequently this effect with it.

Again, M. Prevost remarks, that sometimes an elevation of temperature may be obtained, not only because the air may radiate much heat, but also because, on fine summer nights, the upper air is warmer from having been less cooled (by radiation) than the lower air or the soil.

Hence there are many precautions to be taken to avoid the errors that would result from these unestimated complicated actions. At the same time that experiments are made on the moon's rays, the effect should be observed of other parts of the heavens of equal altitude, terrestrial objects being equally removed in both cases, the temperature of the focus should be compared with the temperature out of the focus, both being equally sheltered from the earth; and there are many points in the manipulation which will suggest themselves to an attentive philosopher, as requiring great attention from the same causes. —*Bib. Univ.* xix. 35.

III. Natural History.

§ Geology, Medicine, &c.

1. *Elevation of the Ocean.*—Among the observations that Mr. Brocchi lately made on the different formations of rocks in Sicily, there is one which merits particular attention. On Mount Pellegrino, near Palermo, he remarked at different heights, above the level of the sea, a number of holes which were formed by the mytilus lithophagus; they are often to be met with in such large quantities, that they resemble honeycombs. They were found even to the summit of the mountain, which, according to the observation of the astronomers of Palermo, rises 1850 French feet above the level of the Mediterranean, and proves how much that sea must have subsided. M. Brocchi observed, that this mountain was a complete *nilometer*. He also says, that he has met with the same appearance at the height of thirty and forty feet above the sea in Hither Calabria, between Fuscaldo and Scalea, towards the promontory of Palinurus, and also in ulterior Calabria on the coast of the Ionian sea.

2. *Encroachment of the Sea.*—On the east coast of America the sea appears to encroach upon the land more and more, from north to south. At Cape May, where the Delaware falls into the Atlantic ocean, a house is built, on the wall of which are inscribed the following important observations:

Distance of the sea from the house.

	Feet
1804	334
1806	324
1807	294
1808	273
1809	267
1811	259

	Feet
1812	254
1816	225
1117	214
1818	204
1819	188
1820	180

The inhabitants of the coast of Brazil say, that they have made similar observations, but we have no particulars of them. There is a building at Ilheos, which was formerly at a good distance from the sea-shore, but is now scarcely a hundred steps from the breakers.—*New Monthly Mag.* vi. 69.

3. *On the Rock-crystal of Primitive Marble.*—The following account of rock-crystal found in primitive marble, is from an essay on the African Alps, and the marble of Carrara, by M. Ripetti, of Florence. If the whole is well authenticated, and the editors of the *Bibliothèque Universelle,* who are certainly competent judges, appear to think it is, it offers some very extraordinary facts.

These crystals are not found indiscriminately in all the Carrara marble, but only in that which belongs to the three excavations of the valley of Ranello, near the foot of Monte Sacro. They are met with either in hollow geodes very completely closed, and against the sides of which the crystals lie irregularly as in siliceous geodes properly so called, or they lie in an isolated state in the mass of marble. In the last case they are frequently opaque and of irregular form, and it is said of them that they have not had space. Crystals of calcareous spar are frequently found in their vicinity, which are called *lucica* by the workmen, and they are considered such certain indications of the neighbourhood of rock-crystal, as to have received the epithet of *spies.*

The author then says, " there is frequently found in the hollow crystalline cavities at Carrara and elsewhere, a very limpid liquid, slightly sapid, and more or less abundant. I have lately had occasion to verify this fact, very common as I have been told in the valley of the Upper Pianello. There I have not only found in these cavities prismatic crystals, but I have seen a very transparent and slightly acid liquid come out of them as the workmen had said; and, who further add, that they sometimes find it in such quantity as to quench their thirst with it, being led to the place by the appearance of the *lucica.* This is not all, and that which follows is still more extraordinary. In the spring of 1819, M. del Nero, proprietor of an excavation in the *Fossa de l'Angelo,* being engaged in sawing out the shaft of a large column of the required length for the temple of St. François at Naples, discovered a *lucica*

in the interior of the marble, about which the workmen penetrated the rock, and found a cavity much larger than usual, lined with crystals, and containing above a pound and a half of the fluid in question. In this cavity there was seen also with astonishment a protuberance as large as the fist, transparent, and which appeared to have all the other characters of rock crystal. M. del Nero delighted to find himself possessor of one of the finest specimens of hyalin quartz which had ever been seen in the country, endeavoured to raise it at the base, but to his inexpressible surprise he found an elastic and pasty substance, which took any form he pleased under his hand, but which was not long becoming hard, and taking on the appearance of calcedony or porcelain. Provoked by the circumstance, the proprietor threw it from him amongst the fragments of the rocks, where this specimen which would have interested the curious so highly, was lost. He affirmed to me, and his assertion was repeated by other witnesses worthy of confidence, that the same fact has occurred more than once, and I made him promise that if he again had the opportunity, he would impress some seal on the soft matter, and when hard send it to me at Florence, with the water which the cavity might contain."

This is a very extraordinary account, but it is given with every appearance of ingenuousness and conviction.—*Bib. Univ.* xviii. 203.

4. *Phenomenon attending the Earthquake at Zante.*—At the time when the desolating earthquake that occurred in Zante, in the end of 1820 took place, a remarkable circumstance was observed just preceding the shock. Three or four minutes before there was seen at the distance of two miles from the point or promontory of Geraca, which is to the S.E. of the island, a kind of meteor burning and almost swimming on the sea, and which continued luminous five or six minutes. At the distance from which it was seen, it seemed to be five or six feet in diameter. Could this be hydrogen gas emanating from some volcanic submarine cavern, and which issuing out of the water in an aëriform column, sought to come in contact with the electricity of the atmosphere? This gas taking fire, continued to burn till the inflammable matter was consumed.—*Edin. Phil. Jour.* vi. 22.

5. *Preservation of Anatomical Specimens.*—Dr. Macartney of the Dublin University, has for some time employed a solution of alum and nitre, for the purpose of preserving anatomical preparations. He finds that it preserves the natural appearances of most parts of the body more completely than spirits or any other fluid heretofore used. The proportions of the

alum and the nitre, and the strength of the solution require to be varied according to circumstances; and, in order to thoroughly impregnate the anatomical preparation, the liquor must for some time be occasionally renewed. The solution possesses such antiseptic powers, that the most putrid and offensive animal substances are rendered perfectly free from fœtor by it in a few days.—*Med. Rep.* xvii. p. 169.

6. *Use of Phosphoric Acid in Jaundice.*—Dr. Caleb Miller has in Silliman's *Journal*, stated the success he obtained in cases of jaundice, by the use of phosphoric acid. His practice is to give a cathartic of calomel and julip, or some of the neutral salts, and then balm-tea moderately acidulated with the phosphoric acid, which is to be continued till it operates as a diuretic, and until the urine becomes clear, or nearly so. One patient had taken eight pints in twenty-four hours. In general the yellowness disappears in three or four days from the urine, and in a few days more from the skin. Dr. Miller has met with but one case, (a person eighty years of age), that had not yielded to this treatment.

7. *Use of Sub-nitrate of Bismuth in Intermittent Fever.*—Dr. Henkesew, a physician at Hildesheim, has been in the habit of prescribing this remedy in agues, for several years. He considers it to be a powerful febrifuge and anti-spasmodic. He exhibits this salt in the dose of four grains, with a few grains of sugar every two hours.

8. *Height of the Mountains in Owhyee and Mowee.*—Captain Kotzebue found the height of these enormous masses to be as follows:

Island of Owhyee—	Merino Roa . .	2482.4 toises.
	Merino Kaah .	2180.1 ——
	Merino Wororai	1687.1 ——
Island of Mowee—	Highest peak .	1669.1 ——

9. *On the Existence of Mercury in the waters of the Ocean.* By M. Proust.—1. Hilaire Rouëlle remarked, a long while ago, that whenever he purified the crude salt of the custom-house in silver basins, they became covered here and there with those spots which are particular to mercury.

2. The same salt, decomposed by sulphuric acid, always gave, in the top of the retorts, small quantities of a sublimate decidedly mercurial.

3. The fact generally known of whitening yellow metals, by putting them for some time into crude or rough salt, added to the preceding results, determined Rouëlle to announce that there was no doubt of the existence of mercury in marine salt.

4. Among the preparations which came to me from Paris to Spain, to furnish the laboratory of the artillery there, were a dozen bottles of fuming muriatic acid, which had been prepared in the laboratory of the druggist, Charlard: all these bottles contained mercury. I at first perceived it, from an amalgam of tin and mercury being left on dissolving some tin in the acid; and afterwards I directly ascertained its presence by purifying the acid in the usual manner, and examining the residuum left; it contained mercury mixed with oxide of iron. It was sufficient indeed to pour a few drops of the proto-muriate of tin into the acid to precipitate the mercury in powder. Hence, then, mercury incontestably exists in the custom-house salt of France.

5. In Spain, the government put to sale the rock-salt of the mines of Cordova and Minglanilla. The first time that I purified the salt sold at Madrid, in a silver basin, I remarked the same spots as those noticed by Rouëlle.

6. Having used all the muriatic acid of Paris, I procured some from the manufactory of acids at Cadahalso. That which was sent to me, had been procured by means of calcined clay. It contained iron, and, to my surprise, mercury also. From that time I remarked, in my course of lectures, the singular accordance between the salt of France and Spain in this respect.

The presence of mercury in rock salt is not astonishing, but when it is found also in the salt produced by evaporation from sea water, there is greater difficulty, because it must be supposed to be in solution. There needs not, I think, more facts or better proved ones, than those mentioned to establish with certainty the existence of mercury in the sea water of this period, and that it has existed also in those which, by evaporation or condensation, have given rise to the deposits of rock salt. All the chemists of the last century, but one, speak of the mercury of marine salt, probably from observations analogous to those I have mentioned, and Rouëlle remarked it before me.

There is nothing as yet demonstrated in the origin of rock salt; nevertheless, if it should ultimately be proved that the principal known mines contain mercury, it would be a new demonstration that the waters of the ocean had concurred in producing them; a consequence which has been already drawn from the discovery of potash in the waters of the ocean, and in rock salt.

An experiment I have desired to make for a long time, but for which the opportunity has not yet offered, is to attach a plate of gold, of two or three inches surface, to some part of a ship, where it would be continually plunged in the water. Half an ounce of gold laminated, would be amply sufficient; all that is required is, to ascertain whether, at the end of a long

voyage, it had become amalgamated; but the person who would successfully make the experiment must not forget that if the plate is lost, it is probably because it would readily detach itself, nothing scarcely being so fragile as gold penetrated by mercury. As to the expense, I shall readily bear it with pleasure. Application may be made to M. Lucas, agent of the Institute, who will immediately give the value to any person willing to take charge of the experiment.—*Memoires du Muséum*, vii. 479.

Art. XXI.—METEOROLOGICAL DIARY for the Months of December, 1821, and January and February, 1822, kept at Earl Spencer's Seat at Althorp, in Northamptonshire. The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For December, 1821.

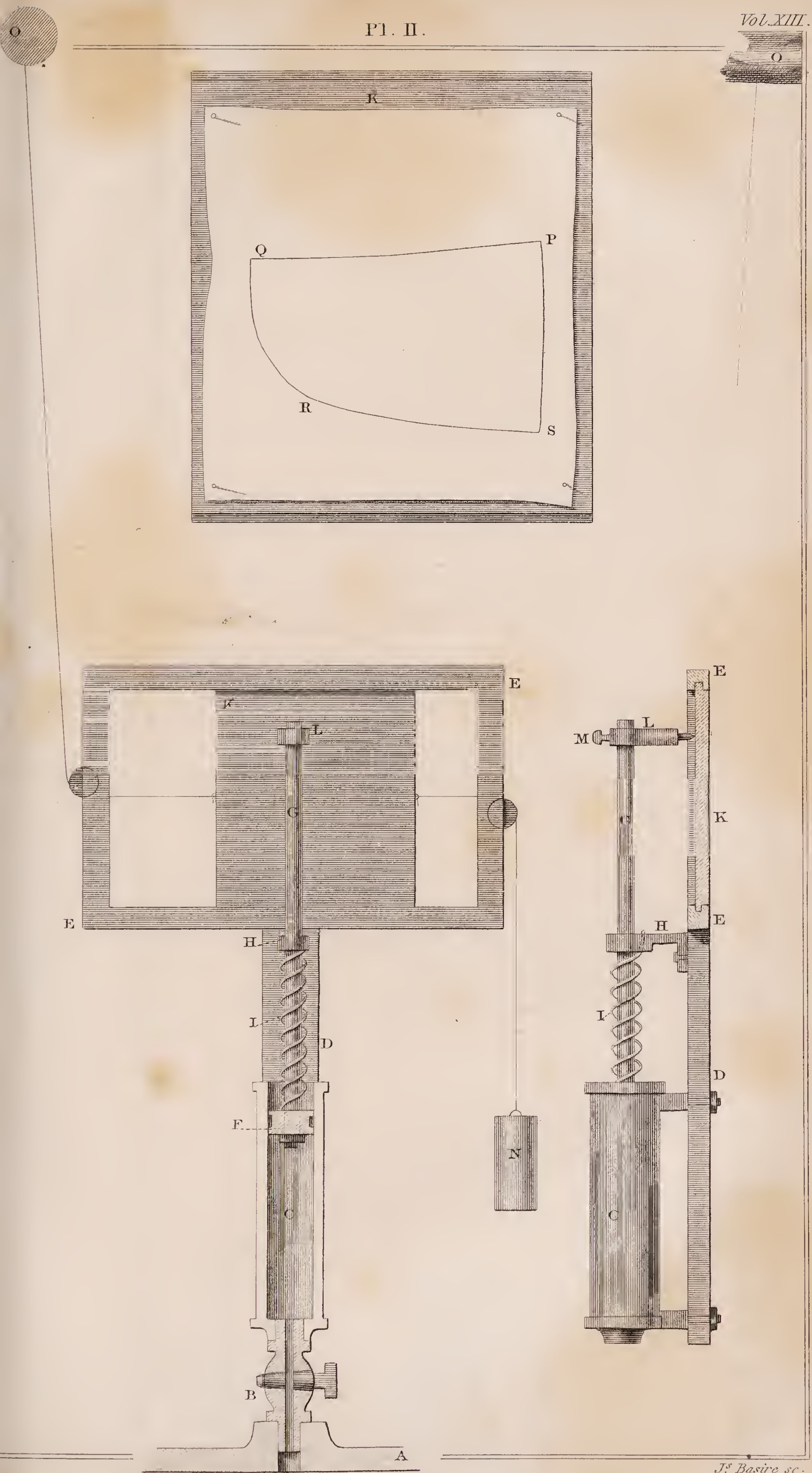
		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Saturday	1	38	46	29.39	29.54	W	W
Sunday	2	36	48	29.69	29.80	W	W
Monday	3	35	49	29.58	29.52	SW	SSE
Tuesday	4	33	46	29.70	29.53	SW	NW
Wednesday	5	43	48	29.60	29.70	WbS	W
Thursday	6	33	39	30.01	30.10	W	E
Friday	7	32.5	43	29.91	29.62	ESE	SE
Saturday	8	42	49	29.80	29.90	WbS	W
Sunday	9	46	52	29.90	29.90	SW	SW
Monday	10	47	53	29.87	29.77	SW	S
Tuesday	11	36	42	30.00	30.18	W	W
Wednesday	12	30	46	30.11	30.01	E	ENE
Thursday	13	45	50	29.80	29.78	SE	SE
Friday	14	33	49	29.82	29.77	SW	ENE
Saturday	15	47	52	29.79	29.79	SE	SE
Sunday	16	45	55	29.60	29.54	SSE	ENE
Monday	17	48	51	29.30	29.30	SW	NE
Tuesday	18	42	47	28.92	28.90	SW	SbE
Wednesday	19	40	46	28.92	29.10	S	SW
Thursday	20	33	42	29.22	29.00	WbS	SE
Friday	21	36	47	28.67	29.13	W	SW
Saturday	22	37	46	29.27	29.15	WSW	EbS
Sunday	23	36	42	28.80	29.00	SW	WbS
Monday	24	34	43	29.79	28.43	SE	NE
Tuesday	25	35	44	28.19	28.42	W	SW
Wednesday	26	28	36	28.40	28.40	NE	WbS
Thursday	27	30	43	28.70	28.89	S	W
Friday	28	36	41	28.60	28.23	SE	E
Saturday	29	39	44.5	28.23	28.63	NNE	SW
Sunday	30	42	43	29.00	29.33	NW	NW
Monday	31	32	39	29.77	29.89	WbN	NW

For January, 1822.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Tuesday	1	30	43	29.58	29.50	SE	SW
Wednesday	2	32	39	29.48	29.66	WbN	SW
Thursday	3	31.5	40	29.77	29.44	W	SE
Friday	4	34	39	29.18	29.43	NE	NE
Saturday	5	32	36	29.82	29.89	N	N
Sunday	6	30	37	29.94	29.99	N	NW
Monday	7	28	36.5	29.85	29.96	NW	NW
Tuesday	8	32	37	30.02	29.89	WbS	WbS
Wednesday	9	34	42	30.09	30.09	NW	NE
Thursday	10	34.5	41.5	29.99	29.99	W	W
Friday	11	38	47	30.10	30.14	W	WbN
Saturday	12	42	48	30.18	30.20	WNW	SW
Sunday	13	44	50	30.16	30.14	W	SW
Monday	14	39	45	30.00	30.10	WbN	SW
Tuesday	15	36	40	30.00	30.12	NW	NW
Wednesday	16	28	35.5	30.07	30.00	W	NW
Thursday	17	27	39	30.00	30.16	SW	W
Friday	18	34	44	30.16	30.20	WbN	SW
Saturday	19	40	45	30.24	30.23	W	SW
Sunday	20	43	50	30.00	30.08	W	W
Monday	21	34	47	30.16	30.24	WbN	W
Tuesday	22	34.5	46	30.30	30.30	W	WbS
Wednesday	23	42	48	30.15	30.08	WbS	SW
Thursday	24	41	50	29.78	29.75	SSW	SW
Friday	25	40	46	29.90	29.93	W	NW
Saturday	26	41	46.5	29.95	29.99	NW	NW
Sunday	27	30	43	30.20	30.19	W	SW
Monday	28	41	47	30.06	30.03	WbS	NW
Tuesday	29	33	46	30.05	30.05	W	NNW
Wednesday	30	31	49	30.20	30.20	NW	SW
Thursday	31	30	41	30.14	30.10	W	WbS

For February, 1822.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Friday	1	39	45	30.00	29.87	WbS	SW
Saturday	2	40	50	29.58	29.30	WbS	WbS
Sunday	3	40	47.5	29.32	29.40	SW	SW
Monday	4	28.5	42	29.60	29.37	WbS	SE
Tuesday	5	40	48	29.05	29.38	SSW	SW
Wednesday	6	31.5	41	29.96	29.90	W	SW
Thursday	7	37	49	29.66	29.60	S	SbE
Friday	8	42	45	29.56	29.78	SbW	W
Saturday	9	36	50	29.70	29.63	WbS	S
Sunday	10	45	50.5	29.68	29.68	SE	SE
Monday	11	36	47	29.83	29.90	W	SW
Tuesday	12	30.5	44	30.09	30.10	W	SE
Wednesday	13	31	46	30.02	29.96	SE	SSE
Thursday	14	37	47	29.93	29.90	SE	SE
Friday	15	41	50	29.89	29.90	SW	SW
Saturday	16	34	45	30.14	30.19	W	SE
Sunday	17	45	51	30.20	30.20	W	W
Monday	18	39	52	30.19	30.17	W	WSW
Tuesday	19	39	49	30.24	30.24	W	W
Wednesday	20	38	48	29.90	29.74	W	NW
Thursday	21	51	44	30.15	30.27	NW	WSW
Friday	22	32	46	30.22	30.04	SW	SW
Saturday	23	35	47	30.04	30.10	WbS	W
Sunday	24	40	50	29.99	29.97	WbS	SW
Monday	25	45	53	30.00	30.03	W	W
Tuesday	26	43	50	29.90	29.86	WbS	NW
Wednesday	27	32	45	30.25	30.19	W	NW
Thursday	28	25	46	30.50	30.34	WbN	S



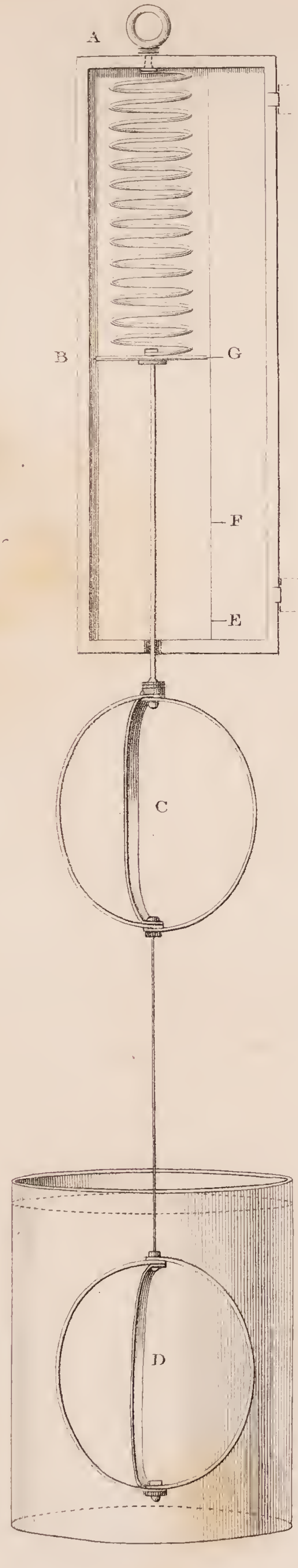
J^s. Basire sc.

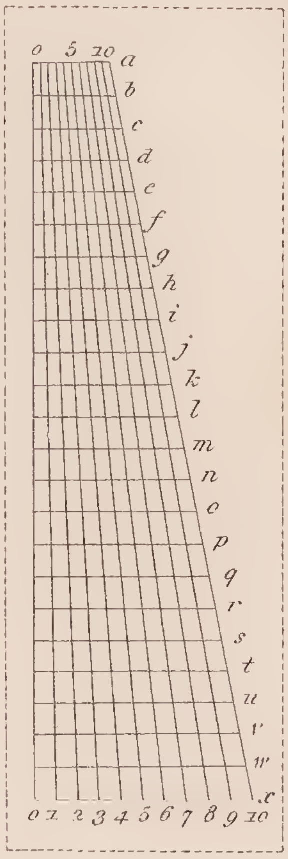
Published by John Murray, Albemarle Street, London. 1822.

A Table shewing the Distances
From the Limb at which the Satellites of Jupiter
immerge into & emerge from his Shadow during 100 days
from the Planets Opposition.
by J. South, F.R.S.

J. Basire sc.

Published by John Murray, Albemarle Street, London, 1822.





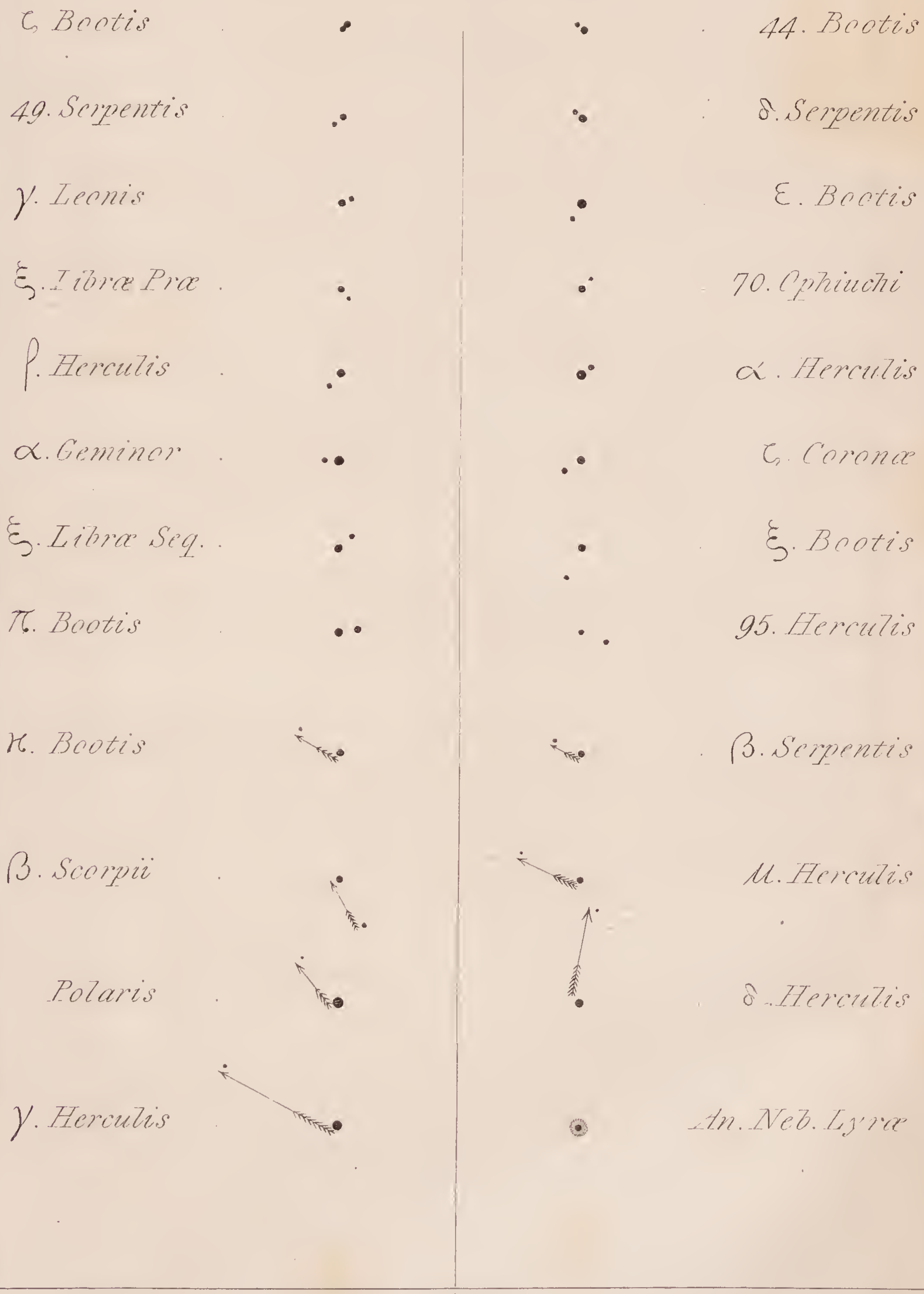
Mr. Creighton's Instrument for ascertaining Specific Gravities.

J^s Basire sc.

Published by John Murray, Albemarle Street, London, 1822.

Diagrams of double Stars, laid down from Observations, with an Achromatic Object Glass, made by Mr. Tulley, of 3 ¾ Inches aperture, having three Convex surfaces, and 45 Inches focal length.

by Jas. South, F. R. S.



Js. Basire sc.

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THE

QUARTERLY JOURNAL,

July, 1822.

Art. I. *On the Desquamation of certain Rocks, and on its Connexion with the concretionary Structure.* By J. Mac Culloch, M.D., F.R.S.

It is well known that many rocks of the trap family undergo a process of desquamation after a long exposure to air, and are thus gradually resolved into crusts which continue to fall off in succession, at length mouldering into clay. The same appearance, although more rarely, occurs also in granite; and in both cases, it has been conceived to depend on an internal concretionary structure, and to indicate the mode in which the constituent parts of the rock are arranged, which, however invisible in the fresh fracture, is thus rendered evident by the progress of decomposition. In the case of the columnar traps, whether basalt or greenstone, this desquamation often proceeds in such a manner from the circumference of a joint towards the centre, that the ultimate result is a spheroidal body.

This effect, compared with that spheroidal concretionary structure which is known to take place in basalt artificially fused, has appeared sufficient to justify the general conclusion, that all appearances of a similar nature depend on the same cause; and, by a slight addition, it has been held sufficient to account for the jointed and columnar structure of the rock in which it occurs. Having met with some remarkable instances in which this effect was decidedly independent of any concretionary structure, I was induced to examine more particularly the nature of the evidence which had been held sufficient to prove

the current explanation; lest, having been proved inadequate to the solution of many cases of this nature, it might turn out purely hypothetical and inapplicable to any. The result has been, that two causes, perfectly distinct from each other, operate in producing the same effect; and it will be useful to state the facts in question, not only as a caution against the universal adoption of a well known rule in philosophizing, and against a hasty mode of generalization, too often adopted in geological science, but as forming an interesting circumstance in the history of the unstratified rocks, trap, and granite.

I was first led to suspect the truth of the current opinion respecting the dependence of the desquamation of rocks on an internal concretionary structure, by casually examining the columns of granite lately brought from Leptis in Africa, and now lying in the great court of the British Museum. I was surprised to find that the shafts of these columns were in the act of desquamation, casting off crusts precisely similar to those which occur in many cases in natural blocks of granite, and equally resembling, except in their superior integrity, those that are found on the surfaces of the columnar trap rocks after exposure to the weather. As it was obvious that the forms of the shafts could bear no relation to the original form of the blocks of granite, from which they had been wrought, it necessarily followed, that in this case at least, the desquamation could not depend on an internal concretionary structure, but must have resulted from the action of the weather on the exposed surfaces.

These specimens being fortunately accessible to many of our geological readers, they will be enabled to confirm, by their own observation, this important fact; nor can the report undergo the hazard of being suspected of inaccuracy or mis-statements; an expedient to which recourse is often had for the purpose of obviating a difficulty, or eluding a fact which militates against some favourite hypothesis, when such a fact is distant or difficult of access; a proceeding which is indeed unfortunately but too much justified by the numerous inaccurate statements in which geological writings abound.

It is worthy of remark, that, in this case, the detached crust is not decomposed, and that, except in tenderness and fragility, it appears scarcely changed from its natural state. Nor is any stratum of clay or decomposed matter found at the place where the crust separates from the solid block; at least none such was found in the places which I had an opportunity of examining. The thickness of the crust appeared to be between the sixth and the eighth of an inch; but it seemed unnecessary to make any accurate measurement of that which is accessible, perhaps to all who may be interested in the subject of this paper.

The decomposition of rocks has, in most cases, and with justice, been attributed to changes in the state of the iron entering into their composition; but it is evident, that neither this, nor the other causes which have been supposed to produce that effect, are capable of explaining the very singular process in question. As it is in vain to speculate on that to which there is no known analogy, I shall not attempt it. It must, nevertheless, be concluded, that this effect cannot arise from that action of the atmosphere which produces the ordinary effect of weathering; nor is it conceivable how it should affect the interior part of the stone, while the surface more immediately exposed to its agency has escaped.

Being in possession of this singular fact, I was induced, as before remarked, to suspect the common opinion respecting the cause of the desquamation of granite, and was therefore led to re-examine with more attention those causes in nature where the same process takes place on the exposed surfaces.

In the greater numbers of these, I only found that which has been observed by every one, namely, that the desquamation of the prismatic or cuboidal masses of granite which are susceptible of this change, took place all round the surface, respecting some imaginary point or centre, and promising, in the progress of time, to reduce the whole to a smaller and more spheroidal mass. Hence, no conclusion could be drawn as to the cause; since the desquamation might in this case be the result of an internal concretionary and laminar structure respecting

one centre, or the consequence of a process similar to that occurring in the columns of Leptis.

But in examining other cases of granitic desquamation, a different appearance was observed. In these it was found, that a single block desquamated in a manner so complicated that no parallelism was maintained between the surfaces of the stone and the crust; and as, in some cases, such blocks were so thoroughly softened as to admit of being cut with a spade, it was easily seen that more than one, or even two centres of desquamation existed in a single mass; the surfaces of the different spheroids interfering with and compressing each other where they came into contact, and the vacuities still required to fill up the solid, consisting of deficient portions of crusts respecting one or other of the approximate imbedded or internal spheroids. It is evident, that in these instances, the effect could not have resulted from that exterior action, be it of whatever nature, which produced the crusts on the columns already described, but must have been determined by other causes depending on an interior spheroidal and concretionary structure; the existence of such a structure being further supported by other well known analogous circumstances in rocks, as well as by those already mentioned which occur in basalt artificially fused, and those which are occasionally found in glass under particular circumstances.

Thus, therefore, it is proved, that granite is susceptible of desquamation from two causes, namely, from an internal concretionary structure of a spheroidal and laminar nature, and in consequence of the exposure of its surfaces to the atmosphere independently of any such structure.

But wherever, in blocks of granite of a cuboidal form, the whole surface desquamates, it has already been shown that it cannot be determined to which of these causes this effect must be assigned; and the same difficulty occurs in those cases where the desquamation takes place in straight laminæ. This is the case of schistose granite, as it has been termed, and it requires a further consideration. As the Island of Arran affords the most accessible examples of schistose granite with which I am acquainted, I shall describe the appearances which it there pre-

sents. Those who may have an opportunity, and who are desirous of examining and verifying this statement, will find it situated in the upper part of Glen Catcol, nearly at the very rise of the stream which flows through that valley.

This granite is of a fine texture, and consists principally of a pale feldspar, intermixed with a moderate proportion of quartz; small crystals of hornblende being sparingly interspersed. It is disposed most generally in extended laminæ of large dimensions, but is also occasionally prismatic; the prisms being however very irregular, and commonly of a small size.

The schistose specimens, or the examples of desquamation, which are here the principal objects of notice, do not occur in all the granite at this place; nor is there any apparent cause for its capricious occurrence, as it is sometimes present, and at others wanting, where the eye does not trace any differences in the appearance or composition of the mass.

It does not often happen that more than one laminæ can be detached from a block; and when removed, no further indication of a similar tendency is in general to be discovered. Neither does the forcible fracture of these blocks detect the slightest indication of a laminar structure; the whole appearing as uniform as any granite which is not marked by this remarkable character. It must also be observed, that there is no disposition in integrant parts of the granite which can account for this desquamation. It does not bear the slightest resemblance even to those specimens of gneiss which approach most nearly to the granite character, nor is there the least indication of a foliated disposition in the integrant minerals; the whole texture being as uniform in the detached laminæ as in the block, and being indeed peculiarly compact.

Although two schists, or laminæ, cannot be detached in succession from one surface, there is sometimes the commencement of a second, indicated by a line on the exterior side, into which the blade of a knife can be insinuated for a small space. But it is more common to find this indication alone; the laminæ previously formed, having fallen off and been washed away

by the rains or streams; producing fragments and gravel, among which imperfect and broken specimens are easily procured. It is now necessary to add, that in these cases, the incipient desquamation cannot be completed by force. Neither, on breaking through the whole block, where the partial division of the laminæ from it terminates, could it be suspected that the process would continue further; as no change whatever is perceptible at the part where, without doubt, it is hereafter to be prolonged.

In examining the detached laminæ, the texture and appearance seem in no way different from those of the solid mass whence it was removed. Nor, as far as can be judged, is it less tenacious than an artificial lamina of the same thickness would be; appearing indeed in every respect perfectly natural. Neither does the surface exhibit any signs of decomposition, being on the contrary brilliant and clean, as if cut by art; even the feldspar scarcely assuming a perceptible degree of that opake whiteness which this mineral so generally puts on after exposure to air. The same cleanness and freshness of both the surfaces in contact, are also found where the laminæ and the block are separated; nor is there any loose matter generated, or any appearances of decomposition in the plane which disjoins them.

With respect to the thickness of the laminæ, it was found to vary from the eighth to the third of an inch, but it most commonly ranged from the sixth to the quarter. Although sometimes of a constant dimension throughout, they are observed to be more frequently variable in thickness; having a greater dimension at one side than another, or the surfaces being inclined. The greatest superficial extent of the laminæ which was observed, did not exceed eighteen or twenty square inches, but, from their brittleness and thickness, they can rarely be procured of half that size.

It now remains to inquire, as already stated, whether this process of desquamation is the result of an internal schistose structure invisible in the solid block, as it is in those cases where it is spheroidal from different centres, and only called into action by exposure to the atmosphere, or whether, as in the case

of the columns of Leptis, it arises from some unknown effect of the weather on the surfaces of rocks possessing no original peculiar structure of this nature.

If we were to form a judgment from merely comparing the appearances in the case of the schistose granite, and in that of the columns, we should be inclined to refer them both to the same mysterious cause. The appearances in both cases are precisely similar, and, in both, they differ from that case in which the desquamation is demonstrated to be the result of an internal concretionary structure; since, in this, it invariably happens that the scales are not only separated, but decomposed, losing their integrity, and falling to powder and gravel on the slightest injury. In this case also, the desquamation and decomposition affect many successive lamina, proceeding in some cases, indeed, even to the central point, or to the heart of the block: whereas, in the case of the schistose granite, as in that of the artificial columns, only one lamina, or a minute space beneath the immediate surface, is affected.

Those who are in haste to establish laws and complete theories, little concerned whether or not they are well founded, will be content to repose in this conclusion. But those, to whom truth has a value superior to all other considerations, will prefer doubt to decision, while any ground for hesitation remains. It has already been stated, that in the columns at the British Museum, the desquamation took place over the whole surface, so as to have produced, could it have been completed all round, a hollow cylinder. The same occurs in the decomposition of those natural blocks where the laminæ refer to one centre only. But in the case of the schistose granite, it must be observed, that the desquamation only took place on one surface, and that surface parallel to the chief planes of the great laminæ in which the granite is disposed. No desquamation at right angles to this was observed; or while the larger planes gave off scales, the narrower lateral ones were free from them. This should not have happened had the effect arisen from the same cause, as in the artificial columns; but every exposed surface should have desquamated alike. On the contrary, the desquamation was

parallel, as already observed, to the great laminar structure; and hence it might be concluded, that both originated in the same cause, or that the great concretionary structure which formed the large lamina, was also the cause which influenced the desquamation of the small one. In this state of doubt must this subject at present remain.

It has, however, been sufficiently proved, that the process of desquamation in granite does arise from two causes; but to which of those two the case of the schistose granite is to be referred, must be a subject for future investigation. The facts are in themselves singular; the most remarkable have never yet been noticed, nor have those which were formerly known been examined with the attention they deserve. They have carelessly been referred to one cause, and the present discussion will have its use, even though it has not cleared up the difficulties of the subject, by turning the attention of geologists to one among many of those more recondite points in the history of rocks, from the study of which geology has already derived, and is probably destined to derive, much light.

I shall now proceed to examine some cases in the rocks of the trap family, which are of a similar nature, and which have been equally neglected; as in these also the appearances in question have invariably been referred to that one cause already shown to be far from universal, namely, a peculiar concretionary structure. It will be seen, that although some of the examples of this occurrence in the trap rocks, are truly dependant on this cause, others are as unquestionably the result of actions similar to those which produce the desquamation in artificial blocks of granite. Thus they confirm the views, already held out, of one common effect proceeding from two causes, while they also offer another analogy to add to the numerous resemblances which exist between granite and the rocks of the trap family.

I need not call to the reader's attention the cases in which columnar traps, whether basalt or green-stone, are known to desquamate, as they are innumerable, and as instances of them cannot fail to be known to all those who have any practical acquaintance with the rocks of this family. In these cases, the

crusts fall off in succession, in such a manner that the angular or prismatic form at length disappears; and the ultimate result is therefore a spheroidal body, destined ultimately to be also resolved into clay. It most frequently happens, that as the crusts are separated they also fall to pieces, or become loose clay; but occasionally they retain a considerable degree of tenacity, although from the changes of their colour from the natural dark blue to brown, it is evident that the iron has undergone a chemical alteration, being converted from protoxide to rust.

Where similar rocks possess a rude and imperfect prismatic structure, the same effects also take place; the surfaces of the prisms desquamating in such a manner as to leave a congeries of spheroidal or ellipsoidal bodies, destined, in the same way, to be ultimately resolved into loose earth.

But these changes are not limited to prismatic trap only, since they occur in other and different forms; being attended with the same final result, whatever the shape of the block may have been, namely, the ultimate production of spheroids. In some rare instances of this nature, it is remarkable that a loose spheroid is sometimes found, on fracture, to be imbedded in an irregular mass apparently solid, or at least giving no exterior signs of desquamation, and bearing no other marks of change than the loss of the blue and the assumption of the brown colour.

Although, in some of these instances, it might be imagined that the effect of desquamation was produced, as in the case of the artificial granite columns, by the exposure of the surfaces to the air, it will, I believe, be found that, in nearly all, they truly depend on an internal concretionary structure. In the case of aggregated columns, for example, the contact of the prism is generally so complete that neither water nor air can gain access to the surfaces.

The same rule sometimes holds with respect to the joints; although, in many of these, perhaps in most, the contact is not so perfect as to exclude either air or water, as is proved by the changes of colour which their surfaces exhibit. But the chief

argument for an internal structure as the cause of the desquamation of prisms, will be found to consist in the effects which take place in those that are not joined, as well as in those masses of trap which affect a prismatic fracture without being absolutely divided into prismatic forms.

In these it may be observed, as in some of the cases of exfoliating granite formerly cited, that the desquamation is of a complicated nature, referring to more than one centre. Thus, in a single unjointed prism, the same result takes place as in those with joints; numerous spheroids being formed in its length by the process of desquamation. The same effect is produced in those irregular masses that are merely characterized by a prismatic fracture, as the exfoliation commences in many different places, referable to different points, so as to leave, in the same way, a number of spheroidal bodies imbedded in a mass of loose crusts and clay.

Indeed it is further often to be observed, that, in cuboidal or otherwise irregular blocks which have neither prismatic form nor tendency, there are several centres of exfoliation; numerous balls being then finally extricated from a single solid block, of which all the surfaces are equally exposed to the action or con tact of the atmosphere.

It appears therefore unquestionable, that in all the instances now enumerated, where the exfoliations respect one or more centres, and the detached crusts are therefore of spheroidal forms, the original cause consists in a spheroidal concretionary structure in the rock, of a nature more or less complicated, although we are equally far from being able to explain in what manner the action of the elements operates on that structure so as to produce the peculiar effects in question. In terminating this part of the subject, I shall quote an analogy in favour of the existence of this structure in prismatic traps, which being, in other respects, of an interesting nature, I purpose to give a more full account of it at some future period.

In this case, which is found in an argillaceous sand-stone, a large bed of that rock is in some places not only marked by a tendency to prismatic division at right angles to the plane of

the bed, but it is actually divided into columns as regular as those which are found in the trap rock, and bearing an average diameter of about two feet. On examining the transverse sections of these columns, it is found that many of them are marked by concentric lines of red and white, alternately preserving a circular form near the axis of the column, but gradually losing it so as to become angular towards the margin where the approximate columns begin to interfere with each other. Even in some part of this sand-stone bed, where no actual division into columns has taken place, the same sets of concentric groups of lines are to be found; each set deviating from the circular form as it begins to interfere with the surrounding ones, and indicating a future possible division of the whole into prismatic shapes. Here no action of the atmosphere can be supposed to have produced this effect, which, on the contrary, marks an original concretionary disposition in the rock, analogous to that which, in all probability, exists in the columnar traps, but here rendered visible by the differences of colour, as it is there concealed by the uniformity of tint.

Having thus proved that the spheroidal exfoliation of trap may proceed from a peculiar concretionary structure, as has been generally received among geologists, I was desirous of ascertaining whether cases might not also exist in which, as in the artificial forms of granite before described, it was due to the action of the atmosphere alone, and independent of any internal arrangement of the parts of the rock. It was evident that this could only be done by examining blocks which had received their forms by art, or by accidental fractures, and after no long search with this view, numerous opportunities were afforded in several places where such blocks of basalt and greenstone had been broken for the purposes of building agricultural dykes.

In many cases of trap, as of granite, no exfoliating tendency seems to exist, and therefore it may often be fruitless to search for illustrations of this subject. But in those to which I now allude, it invariably happened that the blocks used in the buildings, which, from their antiquity, or other causes, had sub-

sequently been demolished, showed the same tendency to exfoliation on all the exposed surfaces, into whatever forms they might have been broken, and whatever their size might be. Thus, in the smaller fragments, the process had already been carried so far as to have solid balls covered with a succession of crusts easily detached; while, in the larger, it was still going on, and promising ultimately to proceed to the same termination. It is plain that, in the case of irregular fragments so formed, no concretionary structure could be suspected; as it is not within the limits of possibility that they should have been broken from the larger masses in such a fortuitous manner as that the centre of the fragment should have coincided with a concretionary centre.

It must therefore be concluded, as enumerated at the commencement of this division of the subject, that in trap as well as in granite, two causes may act in producing the spheroidal desquamation, namely, an internal correspondent structure, aided probably by the action of the atmosphere, and the operation of that agent itself on the exposed surfaces of the same rocks, independent of any such original arrangement.

Having in the former part of this paper examined the nature of schistose or exfoliating granite, and attempted, although fruitlessly, to determine to which of the two cases under consideration this effect was to be attributed, it will render this subject more complete, to extend this inquiry to the case of schistose, or straight laminar trap rock. If, in this case, also, it appears as yet impossible to assign in all cases the true cause, the investigation will not be without use. No distinct account has yet been given in geological writings of this curious and important circumstance in the history of the trap rocks, nor have the various forms under which it appears been described; while it is also certain that some mistaken views have been entertained respecting it, as if it was limited to one species of trap in which it formed an essential character, and which has been very improperly distinguished by this casual and merely incidental circumstance, so as to be erected into a species under the name of porphyry slate. A more accurate description of these ap-

pearances will induce geologists to turn their attention to the actual investigation of that which they have hitherto taken for granted without examination; and possibly enable them to assign the true causes of those circumstances which the writer of this paper professes himself unable to explain.

I have here used the general term trap, merely to avoid circumlocution, as including, together with the greenstones and basalts most commonly designated by this name, all the overlying unstratified rocks of whatever nature. It thus comprises even the porphyries and syenites; but the names of the several species are also given wherever it has appeared necessary to speak more definitely respecting them.

The tendency to schistose, or flat laminar exfoliation on the surfaces, is more common in the rocks of this extensive family, than the spheroidal, since it occurs in every species that I have examined, whereas the latter is rarely found except in basalts and greenstones. An internal flat laminar structure, which seems independent of the agency of the atmosphere, seems also to exist in a greater number of species than the spheroidal concretionary form does, and I shall first describe the cases in which this modification occurs.

I have as yet observed examples of a flat laminar structure pervading a whole mass, only in veins and in prisms; and, in no instance, has a similar appearance occurred in amorphous masses where it was not referable to the other cases of superficial exfoliation, since it never penetrated deep into the mass. It is not uncommon in the veins of porphyry with a base of compact feldspar, which seem chiefly to exist in the primary strata; and, in those, as it occupies the whole breadth of the vein, to which it is moreover parallel, it appears independent of the action of the atmosphere. It occurs also in veins of basalt and of greenstone; and, in some of these cases, veins of very considerable breadth are found to put on this appearance throughout. In all these cases, the thickness of the laminæ varies from the eighth to the third of an inch, or more; and they are separated from each other more or less perfectly, but always without any signs of intermediate decomposition. Although no change

of texture is visible between approximate laminæ, this structure seems to bear an analogy to that larger laminar structure not unfrequent among these veins, in which the several laminæ present different characters; some being plain, others porphyritic, and a third set amygdaloidal. As no action of the atmosphere can be considered capable of producing the effect of a laminar division, thus deep seated and parallel to the walls of a vein, and on account of the analogy just noticed, there is reason to conclude that all these cases are true examples of a concretionary laminar structure.

The most remarkable instance of a flat luminar structure pervading a mass of prismatic form, occurs in Sky, in a greenstone of fine texture; and, in this case, the division is parallel to the axes of the prisms. It bears no resemblance therefore in form to the spheroidal exfoliation of columns formerly noticed; as it does not take place at all on the transverse surfaces, while it also occurs, in greater or less perfection, to such a depth within the stone, as to be apparently free from the influence of external causes. It must also be remarked, that, both in this instance and in that of the laminar veins, the integrity of the structure is in no way affected, but that the laminæ retain even the original colour of the massive parts of the rock.

It appears therefore reasonable to conclude, that, in this case also, as in the former, the laminar division parallel to the axes of the columns, depends primarily upon an original structure of that nature, which is developed, in all probability, by some action of air or water, or both, analogous to that which, in some of the cases formerly enumerated, seems capable of producing it upon the exposed surfaces.

I must now enumerate those cases of a laminar exfoliation, of much more frequent occurrence, the cause of which is far less easy to assign, and which, although in some instances they may be the consequences of a peculiar original structure, appear often to be produced, as in some of the cases of granite and others of spheroidal exfoliation in the trap rocks, by the mere action of the atmosphere on the flat exposed surfaces.

This phenomenon may be witnessed in so many parts of the

western islands of Scotland, that it is unnecessary to particularize the instances. I have observed it in clink-stone and in clay-stone, as well as in the various porphyries which have these ingredients for a base, and in the syenites.

In one or two instances it occurs in a clay-stone which has a columnar form; and here, contrary to the case described as found in Sky, the laminar exfoliation is at right angles to the axes of the prisms. It is proper to remark, however, that in the only examples which came under my notice, the formation of the laminæ was superficial; there being rarely more than the indication of three or four divisions, and not above one or two of these being so far separated as to admit of being removed. In no one case of this, or of the following occurrence of a laminar exfoliation of shapeless masses, did I observe that the deeper parts of the rock were affected, or that this structure could be inferred to pervade the whole.

In the above-mentioned cases also, where the exfoliation took place at right angles to the axes of the columnar claystones and porphyries, it is proper to remark that these were the only surfaces exposed. Yet it is not improbable, that future observers will find that the sides as well as the ends of such columns may present this appearance, to whatever cause it may be owing, whether to simple exposure or to an internal corresponding structure.

As the rocks now enumerated are much more commonly found in an irregular than in a columnar form, so the instances of schistose exfoliation are sufficiently frequent in these to admit of their examination in almost all situations where rocks of this nature occur.

The only one of these rocks which presents a large laminar disposition analogous to that of granite, is hypersthene rock. In this, the exfoliation in question sometimes occurs on the surface; but it is always limited to one scale, which, in its thickness and tenacity, and in the general appearance at the plane of separation, exactly resembles that which occurs both in the schistose granite of Arran, and in the columns from Leptis, already so often mentioned.

Few marks of such a large laminar disposition are to be observed in the syenites, clay-stones, and clink-stones, or the porphyries derived from these two latter rocks; although the Isle of Mull presents some remarkable exceptions. No relation can therefore be inferred between the direction of the exfoliation and that of a larger mass; and accordingly we can only conclude that, whatever surface is thus found exfoliating, the direction of this change relates to that of the exposed surface. Yet, as it will, I believe, always be observed, that none of these rocks exfoliate in two directions at any one place, it is probable that this tendency does actually bear some relation to the internal structure of a mass, no other indications of which are visible.

The number of successive laminæ which may be detached from any of the rocks last named, is various; but I know not that it bears any constant relation to the species. I have only observed, that when they do occur, they are more numerous in the softer clay-stones than in the syenites, or rocks with a base of clink-stone. If one, two, or three, can be detached in succession, that facility soon ceases; and, after some partial indicatures of future desquamation, the rock is found to be massive, and to give no marks of the future renewal of a similar process, although there can be little doubt that it will continue in succession as the preceding laminæ become detached. Such laminæ generally present the same average but variable thickness which has already been mentioned. The surfaces are also occasionally undulated or irregular, and, in a few instances, slightly curved. From preserving the tenacity of stone, it is generally supposed that they are unchanged, and present the natural characters of the rock to which they belong. But this is not the fact; and they will be found, in this important circumstance, to differ essentially from those laminæ already described which pervade the interior of veins, and which are conceived to arise from an internal concretionary structure. The deception has in these cases arisen from the great depth to which the rocks of this family sometimes weather without losing their tenacity, undergoing little apparent change but that of colour. Hence the solid parts of such rocks, when broken even

below the exfoliating surfaces, will be found to present the same appearance as the detached scales. But if a deeper section be made, it will, I believe, invariably be found that the natural state of the rock is different, and that the process of exfoliation has been attended by a partial decomposition or change.

I may here briefly remark, while on this subject, that the term *schistose porphyry*, already noticed as applied to the rocks of that character which are susceptible of this change, is injudicious. Such a term might, even with more propriety, be applied to the laminar porphyries with a base of compact feldspar that occur in veins; since in them it appears more truly a natural conformation, and independent of the action of extraneous or accidental causes. But in any case it is improper to erect a species from distinctions which are merely of an accidental nature, and which, if in this case they are not absolutely produced, are still materially influenced by accidental causes. Nor is there any reason why the term schistose should be limited to the porphyries alone that present this character; as there is the same reason for extending it to the clay-stones and syenites that exhibit the same appearances. I have here used the term schistose porphyry in making these animadversions. That of porphyry slate, commonly in use, is still more objectionable; but I need not prolong these remarks, having already noticed this circumstance in my Account of the Western Islands.

It only remains, in concluding this subject, to inquire whether the exfoliation of the laminæ, in these last examples, depends, as in the case of artificial blocks of trap and granite already described, upon the action of the atmosphere alone, or whether it is the consequence of this action coinciding with an internal and original concretionary arrangement. On this subject, it appears to myself that the arguments on each side are pretty nearly balanced. A similar structure of a concretionary nature is proved to exist in some traps, and therefore it may be conceived to exist in these also. On the other hand, it takes place only within the influence of the atmosphere, and is attended with a degree of decomposition which indicates the

influence of air and water on the rock. It seems more prudent to leave this question undecided for future examination; and I shall therefore terminate this paper, satisfied with having placed the facts in such a state as to be ready for any future illustration which geologists may add to the subject.

J. Mac Culloch.

Additional Remarks on the Desquamation of Rocks.

When the foregoing paper was written, I had not observed the desquamation which is there described, in any rocks but those of the families of the Granite and Trap. But an interesting case of this nature having since occurred in one of the primary stratified substances, it has appeared to me worthy of being added as a supplement to that record. It is true that it does not throw any additional light on the immediate cause, or on the mode in which the atmosphere operates in producing this remarkable effect. Yet it proves clearly, as clearly as the case of the columns of Leptis, that this effect is not dependent on any internal concretionary structure in the stone, and that it is not even limited to those rocks which are so often characterized by that peculiarity.

In Glen Almond, a well-known valley, through which the military branch of road from Stirling to Dalnacardoch passes, and which conveys the river Almond from its native hills to the great plain of Strathearn, there is an insulated stone long known by the name of Ossian's Tomb. Whatever interest this belief may excite in the breast of a true Highlander, a geologist is very little concerned with respect to that visionary claim of honour; but, from some indications of a circle of which it is the centre, it appears rather to have been adopted as the central stone of a Druidical structure. From its weight and bulk, it has unquestionably not been placed by art in its present position; but appears, on the contrary, to be a block which, like many others, has fallen from the impending precipices of the hills above.

It corresponds in its nature with these; being a micaceous

schist of a foliated and somewhat undulated structure, and being also characterized by a considerable quantity of quartz proportioned to the mica which enters into its composition. It is a very hard variety of this rock; the quartzose laminæ being compact and of considerable thickness.

From the universally weathered state of all the exposed surfaces, and from the succession of lichens which appear to have grown and decayed on it, a long period must have passed since it quitted the mountain above: if ever applied to the objects of Druidical worship, as is here supposed, we can at any rate assign pretty nearly the minimum of time during which it has lain where it now stands.

Although this stone bears, on all the surfaces, those slight marks of decomposition so well known in micaceous schist, which consists in the rusting of the iron of the mica, or in its assuming a brown hue, it is only on one side that the peculiar effect which forms the object of this paper has taken place. This is the north-eastern, or northern quarter; and, from the form of the valley, it is probably that on which the wind and rain beat with most violence. This circumstance furnishes a proof, if proof were wanting, that the effect, however inexplicable, is the result of atmospheric action.

From nearly the whole of this surface, scales of the rock can be detached, scarcely differing in tenacity and hardness from the original stone, and thus resembling those which have exfoliated from the granite columns described in the former paper. Their thickness varies in different parts, from the sixth of an inch to nearly the half; and they may be obtained, by careful application of a knife or thin chisel, in plates containing from six or eight to twelve or sixteen superficial inches. At the planes where they separate from the mass of the rock, no powdery matter, or grains of sand, or rusty clay, or any other marks of real decomposition are visible; both the surfaces being clean and smooth, as if cut by a sharp tool.

It must further be observed, that there is here a succession of similar operations visible. At some former period, the plates first detached have in some places fallen off, so as to

leave a new surface exposed; and on this the same desquamation has again taken place, so that, in some parts, two successive plates can be separated. The second plate is, however, in this case, not placed under the first, but forms a lower stage or step; its upper surface, if prolonged, being the under one of the first scale. In other cases, it is merely apparent that the process is about to be completed at some future period; the edge of the scale just admitting a knife, but it being as yet possible only to detach a small portion at the edge.

In one place it was also observed, that there was an incipient mark, or indication of a third scale; leaving no doubt that, in a sufficient length of time, the same process may be expected to take place through the whole block, should the same external circumstances continue to act.

This fact would have been in no respect remarkable, if the desquamation had taken place in a direction parallel to the laminar structure of the stone. Where micaceous schist is of such a nature that laminæ of quartz are separated by others of mica, the rusting of the iron in the latter, and its consequent decomposition, easily permit the quartzose laminæ to be detached. But, in this case, the desquamation is at right angles, or, at least, at considerable angles to the laminar structure. Thus it produces a scale, or slate, which consists of parallel bands of quartz and mica; both of them remaining unchanged, as in the solid rock, but easily separated in consequence of the fragility of the micaceous band.

It is evident, therefore, that in this instance the desquamation is not only not produced by the peculiar structure of the rock, but is utterly independent of it. Under the same circumstances, it might equally be expected to occur, either in a mass of pure quartz rock, or in a micaceous schist of a more simple and homogeneous nature; and, in this latter case, the schist might desquamate at angles to its fissile tendency. Such an occurrence might excite surprise; but there is no apparent reason why it might not happen in either of these rocks in a separate state, as it does here where they are intermixed in distinct laminæ.

The peculiar circumstances of predominant exposure to the

weather on the one side of this rock, where the desquamation takes place, contributes to prove, still more clearly than in many of the instances formerly enumerated, that the whole of this process is caused by the action of the atmosphere and the rains. However mysterious it may at present appear, it is the result of some chemical actions which cannot for ever be concealed. How far it may be connected with any other circumstances of the same nature more generally interesting, it is impossible to foresee. But, like all new facts in an obscure science, it is worthy of record. In multiplying the examples of difficulties and obscurities, they become gradually removed from the list of exceptions; while the varieties which are discovered in them on the comparison of many examples, sometimes point out the causes which have influenced the whole.

ART. II. *Account of an Instrument for determining the Specific Gravities of Solid Bodies. By* H. CREIGHTON, Esq.

[In a Letter to the Editor.]

SIR,

SOME time ago, I constructed an instrument for determining the specific gravities of solid bodies, of which the following is a description: I presume the same may be found acceptable to some of the readers of the Quarterly Journal.

The theory of this instrument depends upon the known property of springs, undergoing degrees of extension proportional to the weight or force applied to them, and the screw form usually adopted for weighing of bodies is best adapted to the purpose now in view.

In the accompanying sketch A. B. Plate IV. represents one of these springs suspended at A, and carrying an index at B. its lower extremity: C. D. two cages of curved wires, with joints to fold into a small compass, (or scales similar to those usually attached to the ends of a balance-beam may be used instead); the lower one D. is immersed in a vessel of water (which must be considerably deeper than represented) and both are sus-

pended by fine threads or wires from the end of the spring at B.

The substance whose specific gravity is to be ascertained being placed in C, it will stretch the spring and lower the index, suppose to E, where a mark is to be made; let the body then be transferred to D, and a second mark F. made opposite to where the index now stands. G being the point shewn by the index when the scales are both empty, G. E. ($= \alpha$) will represent the whole weight of the above substance in air; G. F. its weight in water, and E. F. ($= \beta$) the difference of these weights; consequently $\frac{\alpha}{\beta} = S$, the specific gravity required. To find the value of S, take the extent E. F. with a pair of compasses, and by means of them see how oft this is contained in G. E. ascertaining the value of any fractional part by means of a sector, or a scale of diverging lines as represented at H. On a scale of this kind a horizontal line, as k, will be found, equal to the divisor E. F. divided decimally, and with subdivisions, if of sufficient magnitude; here the amount of the above fraction will be .71, and E. F. being twice contained in G. E. 2.71 will be the value of S, or the specific gravity of the substance experimented upon.

Extreme accuracy is perhaps not to be attained by an instrument of this kind without making the spring of an inconveniently large size; but for all common and generally useful purposes it may be employed with advantage. If the spring is of such dimensions that half a pound will without injuring it stretch five or six inches, an error of one in five hundred can scarcely occur, if common care is taken in ascertaining the result.

In the usual way of taking specific gravities, the imperfections of balances sometimes employed, the chances of error in noting down the weights, and in the consequent calculations, however simple, frequently render repetitions necessary, and employ a considerable portion of time. With an apparatus as described above, time will be saved; and as the results are made obvious in a great measure to the eye, as well as to the

understanding, any errors of magnitude are less likely to be made.

I constructed one of these instruments with a spring capable of sustaining about one pound when it was extended six inches. Another required four to five ounces to stretch it through the like space, and might be loaded with six or seven without injury. A third, with spring made of small harpsichord wire, stretched about six inches with 50 grains, and was extremely sensible.

The stiffness of these and similar springs depends not only upon the thickness of the wire of which they are made, the diameter, and the number of the coils, but also in a great measure upon the degree of hardness or temper given to the steel or other material used in their manufacture. The following table (I.) shews the result of some experiments I made with a variety of springs of steel of the same quality, and equally tempered, or as nearly so as could be effected by tempering the whole at one process; the wire was 0.045 inches diameter.

Column 1 is the mean diameter of each spring in inches, and column 2 the number of coils of wire when the springs were in their natural state or not stretched; column 3, the space in inches through which the extremity of each spring was extended by a weight of 10,000 grains.

Table I.

1	2	3
0.41	34.60	0.847
0.45	31.55	0.765
0.60	23.70	2.250
0.70	20.03	2.005
0.80	17.75	2.650
1.22	11.65	7.410
1.42	10.00	9.880

In Table II, (arranged as the last) the springs were of highly tempered steel; in Tables III. and IV. the springs were of hard iron wire, and sustained the weights given without injury.

TABLE II.

With wire 0.06 diam. the weight 30,000 grains.		
0.45 0.72	17.00 10.90	1.20 3.20

TABLE III.

With *iron* wire, .1175 diameter, the weigh 28lbs. Avoidupois.		
0.61 1.18 2.06	34.00 17.60 10.01	0.73 2.60 10.00

TABLE IV.

With *iron* wire, .221 diameter, weight 56lbs. avoirdupois.		
1.60	60.00	5.05

In preparing these springs, care was taken to keep the different coils of each a little asunder; they would not stretch uniformly if allowed to be in contact any where.

The space through which a given weight stretches any of these springs is in proportion to the number of coils contained in it, the diameter being the same; but where the diameters are various, there does not appear to be any regular proportion, though in some cases it was observed to be nearly as the squares of these diameters; the tables, however, may assist in giving a general idea of the size of springs required for any given weight, and in an instrument to be used for weighing or taking specific gravities, as above described, two or three springs might be contained, suited to different weights and divided scales of ounces, grains, &c., added to make it useful as a common spring balance; a very small box or case would contain the whole, and the scale of diverging lines may be put inside the lid or cover.

I have the honour to be, Sir,

Your very obedient Servant,

HENRY CREIGHTON.

W. T. Brande, Esq. R. I.

Art. III. *A Review of some of the General Principles of Physiology, with the practical Results to which they have led*.* *By* A. P. W. Philip, M. D., F. R. S. Edinb.

[Continued from page 113.]

We are now to direct our attention to the phenomena of the nervous system. Under this term is generally included the sensorial as well as nervous system, properly so called. From a careful review of the functions of these systems, however, it will appear, I think, that they do not differ less from each other than from the muscular system.

M. le Gallois, as far as I know, is the only author who has endeavoured by experiments to draw a line of distinction between them. It is unnecessary, however, to examine the opinion he has advanced, as many of the facts which I shall have occasion to state will be found incompatible with it. After reviewing the phenomena of the nervous system, properly so called, we shall be better prepared to enter on this question.

The functions of the nervous are much more complicated than those of the muscular system. The first we shall consider is one on which I have already been necessarily led to make some observations. We have seen that the influence of the nervous system is the only stimulus of the muscles of voluntary motion, and that it is also capable of exciting those of involuntary motion, although in their usual functions the latter are excited by other means. Here the question arises, if the nervous system be not concerned in the usual functions of these muscles, why are they universally subjected to its influence? This question we are not prepared to consider till we have taken a view of some of the other functions of the nervous system;

* Errata in the first part of this paper:—Page 97, l. 22, *for* matter *r.* them. Some infer from the evident dependence of the phenomena of the vital principle on the due mechanism of the living body, that it is the immediate result of that mechanism; but this dependence would not be less, were the vital principle something superadded to bodies, as electricity and magnetism are by many supposed to be. There is nothing in the phenomena of the vital principle which can authorize either inference. Page 110, l. 4, *for* produce *r.* produces.

but the influence of that system not only does not excite the muscles of involuntary motion in their usual functions, but, as I have already had occasion to observe, is communicated to them in a way different from that in which it is communicated to the muscles of voluntary motion. We shall here inquire in what this difference consists.

The following positions have been ascertained by repeated experiments*. Chemical agents, applied to the brain and spinal marrow, more powerfully influenced the heart than mechanical agents, while the latter influence the muscles of voluntary motion more than chemical agents. Both, applied to the brain and spinal marrow, excite the heart after they cease to produce any effect on the muscles of voluntary motion. Applied to any part of the brain and spinal marrow, they affect the action of the heart, while the muscles of voluntary motion are only affected when they are applied to the parts from which the nerves of those muscles originate. Applied to the brain and spinal marrow, they never excite irregular action in the heart, while nothing can be more irregular than the action they excite in the muscles of voluntary motion. Their effect on these muscles is felt chiefly on their first application, but continues on the heart, within certain limits, as long as they are applied. These differences in the effects of agents applied to the brain and spinal marrow must, it is evident, be explained, before we can understand the relation which subsists between the nervous and muscular systems.

It appeared to me probable, from the result of several experiments, that the cause of chemical agents, applied to the brain and spinal marrow, producing a greater effect on the heart than those which act mechanically is, that the former, from their nature, act on a larger surface. If this opinion be correct, the mechanical agent, it is evident, may be rendered the most powerful, by confining the chemical to a smaller space than it occupies, which was found from frequently-repeated experiments to be the case†.

Most of the experiments on this part of the subject, it may be observed, as well as many to which I have already referred,

* *Exper. Inq.*, Part II., Chap. 4. † *Ib.* Exper. 41.

were made, not on the living, but newly dead animal, which was always employed if the nature of the experiment admitted of it*.

It appeared, from repeated experiments, that neither chemical nor mechanical agents, applied to the brain and spinal marrow, affect the action of the heart, unless they make their impression on a large portion of these organs. Every part of them may be stimulated individually, without the action of the heart being influenced; and the agent being the same, its influence on this organ is always proportioned to the extent of surface to which it is applied†. It does not appear that it is of much importance on what part of the brain the agent makes its impression, Even stimulating the surface alone, either mechanically or chemically, immediately increases the action of the heart.

Another circumstance, which appears to be of great consequence in explaining the difference of the effects of agents applied to the brain and spinal marrow on the two sets of muscles, is, that the heart obeys a much less powerful stimulus than the muscles of voluntary motion do. The most powerful chemical agents alone affected them, while all that were tried readily influenced the action of the heart ‡. Mechanical agents which, by bruising and dividing the parts, occasion the greatest possible irritation, are best fitted to excite the muscles of voluntary motion. Chemical agents, indeed, from their effects on the heart, we should, at first view, consider the most powerful. But their greater effect on this organ is readily explained by what has just been said. When the effect of the mechanical agent was rendered extreme and general, its influence on the heart was found much greater than that of any chemical agent which was tried. We have seen that suddenly crushing any considerable portion of the brain or spinal marrow instantly destroys the function of that organ.

The conclusions, then, at which we arrive are, that the heart

* Why the newly dead animal is as good a subject for many physiological experiments as the living one, will appear from what I shall afterwards have occasion to lay before the reader.

† *Exper. Inq.* Exper. 41, 42, 43, ‡ *Ib.* Exper. 35, 36, 37.

is influenced by all agents applied to any considerable part of the brain or spinal marrow, while the muscles of voluntary motion are only influenced by the more powerful agents applied to certain small parts of them.

These facts being ascertained, the other differences observed in the effects of agents applied to the brain and spiral marrow on the heart and muscles of voluntary motion are easily explained.

Irregular action of a muscle arises from stimuli acting partially or at intervals on its nerves, or on the part of the brain or spinal marrow from which its nerves arise; but very partial action of an agent on these organs, we have just seen, is incapable of exciting the heart; and while the agent is applied to any part of them, as all their parts seem equally to influence the heart, it cannot act upon it interruptedly, as an instrument does on the muscles of voluntary motion, when it is moved from place to place in the brain*.

The heart feels the effect of the agent within certain limits, as long as it is applied to the brain and spiral marrow, while the muscles of voluntary motion chiefly feel its first impression; because they feel only the effects of powerful agents, applied to certain small parts of these organs, which, being strongly impressed, soon lose their excitability; while the heart feels the sum of all, even slight impressions, made on every part of them †.

It also appears, from what has been said, why those who have endeavoured to influence the heart by stimulating the small parts of the brain from which its nerves seem chiefly to originate,

* It is true, that although the heart is only influenced by agents applied to a large portion of the brain, we may conceive them so applied as to produce irregular action in it, and we find that certain irritations of the nervous system have this effect. Suddenly crushing part of the brain or spinal marrow renders the action of the heart irregular. But it is evident that the heart, not being subject to agents whose action is confined to a small portion of these organs, and being equally affected through all parts of them, must render it much less subject to irregular action, and readily accounts for this not having been observed in the experiments just referred to.

† *Exper. Inq.*, Exper. 34.

have failed; and why the heart may be influenced through this organ and the spinal marrow, after their power is too far reduced to excite the muscles of voluntary motion*. As these only obey agents applied to one part, if the change there be not sufficiently great to produce the effect, it can be assisted by no other. Thus I have found by experiment, that a blow which affects the brain generally, without materially injuring it, produces comparatively little effect on the muscles of voluntary motion, but it produces a great effect on the heart, because it feels the sum of all the impressions. The nervous system, therefore, may be so far exhausted as not to admit of the vivid impressions necessary to excite the muscles of voluntary motion, and yet capable of those which influence the heart.

The heart, however, is not the only organ which receives nervous influence from every part of the brain and spinal marrow. The power of the blood vessels, we have seen, may be destroyed by the sudden destruction of either of these organs, and it also appears, from direct experiments†, that they may be influenced, even in the extremities, by agents applied even to the upper surface of the brain. The alimentary canal may also be influenced through both the brain and spinal marrow. From the extreme irregularity of the motions of this canal, we cannot ascertain whether it is subject to the influence of the different parts of the brain and spinal marrow, in the way in which this was done respecting the heart and blood vessels. I therefore endeavoured to ascertain this point by experiments of a different kind; from which it appears, that on withdrawing a great part of the influence of either the brain or spinal marrow, the stomach is affected in a way which I shall soon have occasion to consider more particularly‡.

We may easily conceive why the muscles of voluntary motion are excited when those parts of the brain or spinal marrow from which they receive their nerves are stimulated; but it seems at first view more difficult to account for the heart and other

* *Exper. Inq.*, Exper. 38.
† *Exper. Inq.*, Exper. 26, 27.
‡ *Ib.*, Chap. VII. sect. II.

muscles of involuntary motion being subject to the influence of every part of these organs. We cannot suppose that they receive nerves from every part of them. We know, indeed, that no organ does so. The following seems to be the state of the question. We see some parts influenced by every part of the brain and spinal marrow; others only by small parts of them. In the latter instances, we see directly proceeding from those small parts the nerves of the part influenced. In the former instance, namely, where the part is influenced by all parts of the brain and spinal marrow, we do not see nerves going directly from all parts of these organs to the part influenced, but we see this part receiving nerves from a chain of ganglions to which nerves from all parts of them are sent. It is, therefore, evident, from direct experiments, that the nerves issuing from ganglions convey to the parts, to which they send nerves, the influence of all the nerves which are received by these bodies.

Such then is the relation which the most important organs of involuntary motion bear to the brain and spinal marrow. Their powers are not directly dependent on either, yet they are subjected to the influence of every part of both, communicated through the medium of the ganglions; and when we see the other organs of involuntary motion equally independent of the brain and spinal marrow, and supplied with nerves from ganglions, in the same way with the former, it is impossible not to infer, that they bear the same relation to the nervous system. Thus, it would appear, that the ganglions may be regarded as a secondary centre of nervous influence, receiving supplies from all parts of the brain and spiral marrow, and sending to certain organs the influence of all those parts.

If the nervous influence of the thoracic and abdominal viscera be thus supplied from a common source, why, in affections of the spinal marrow, it may be asked, is the breathing most influenced when the disease is in the dorsal portion of this organ, and the action of the bladder and rectum, when its chief seat is in the lumber portion? This necessarily arises from the intercostal muscles deriving their nerves from the dorsal, and the abdominal muscles from the lumber portion of the spinal mar-

row. The latter muscles generally excite, or, at least, increase, the action of the bladder and rectum, by pressing them against their contents, and also by this pressure contribute mechanically to expel their contents. Thus, in the above cases, in addition to the failure of nervous influence in the viscera, there is a failure of excitement in the muscles of voluntary motion which conspire with these viscera in certain parts of their functions.

We can trace the communication of nerves issuing from the great chain of ganglions, placed it would seem, to facilitate these communications in the centre of the animal system, with all the nerves of the body. Bichat, although his opinions respecting the use of the ganglions are inconsistent with the results of the experiments just referred to, as well as of others to which I shall have occasion to refer, was induced, from their situation and the distribution of their nerves, to regard them as the centres of nervous systems.

On comparing all the facts on the subject, we have reason to believe, that the system of ganglionic nerves is quite as extensive as that of the nerves proceeding directly from the brain and spinal marrow. We every where find blood-vessels which, being influenced equally through the brain and spinal marrow, must receive the nervous influence through the ganglions; and, indeed we can trace the ganglionic nerves attached to and supplying the larger vessels. The following case, related by Dr. Parry, in his treatise on the arterial pulse, might alone be regarded as proving the existence of two sets of nerves in the extremities, the one supplying the organs of voluntary, the other those of involuntary, motion, and strikingly illustrates what has been said on this subject. He observes, "I have seen a total loss of pulse in one arm, with coldness, but complete power of motion in that part while the other arm was warm, and possessed a perfectly good pulse, but had lost all power of voluntary motion."

Such then is the manner in which the influence of the nervous system is supplied to the muscles of voluntary and involuntary motion. To the former, from certain small portions of the brain and spinal marrow, and through nerves going directly from these

small portions to the muscles; to the latter, from every part of the brain and spinal marrow through a chain of ganglions which, on the one hand, communicate with every part of these organs, and, on the other, with all the muscles of involuntary motion; in the former instance, the influence of the nervous system being the only natural stimulus, in the latter, other stimuli exciting the muscle to its usual function, and the influence of the nervous system, being only occasionally bestowed on it for purposes which we shall soon have occasion to consider.

When the nerves of a muscle of voluntary motion are divided, the supply of the stimulus on which its function depends being cut off, it is rendered paralytic, not because its power is impaired, for it is as sensible to the effects of stimuli as while its nerves were entire, but because the channel of the only stimulus by which the will operates on it is obstructed, and here the effect of the division of these nerves ends. The consequence is very different, when the nerves of the muscles of involuntary motion, the ganglionic, are divided.

If the principal ganglionic nerves, the eighth pair through which the influence of the brain is chiefly supplied to those muscles be divided, the function of the muscles appears to be wholly unaffected by it. The heart and vessels support the circulation as well as before the division of the nerves. For this result we are prepared by what has been said of these muscles. An evident disorder, however, in the secreting power of some of the vital organs immediately ensues. The stomach no longer secretes a fluid capable of producing the necessary change on the food*, while the fluids of the lungs deviate from the healthy state, and accumulate in the bronchiæ and air cells. The structure of the lungs itself, in the space of a few hours, becomes evidently diseased, and the animal dies of dyspnœa; failure in the office of the lungs, necessarily proving more suddenly fatal than failure in that of the stomach. It has been questioned, whether the effects on secreting surfaces of dividing the eighth pair of nerves should be ascribed to the interruption of the

* *Exp. Inq.* Chap. 5.

influence of the brain, or to the injury done to those surfaces by the act of dividing their nerves*. That it produces its effects in the former of these ways, appears from the following facts. When secreting surfaces are deprived of their nervous influence by any other means, the effect is the same; this effect is not at all proportioned to the degree of injury done to the nerves, but to the degree in which the nervous influence is withdrawn; and, as soon as the nervous influence is restored, the surface is again capable of its function†. These facts seem sufficient to have answered the question, although it had not been determined by some late experiments, in which Mr. Brodie and Mr. Cutler were so good as to assist me‡, that it is necessary, after the division of the nerves, to displace one of the divided ends, in order wholly to arrest the function of the secreting surface, the influence of the brain still passing in such a quantity, if this be not done, as to bestow on that surface a considerable degree of the secreting power; and that even when the divided ends, if not otherwise displaced, are separated to a distance of a quarter of an inch.

Thus, we find, that the effect of dividing the ganglionic nerves is of a nature wholly different from that of dividing the cerebral, or spinal nerves; while the division of the latter only deprives the animal of the power of exciting the muscles of voluntary motion, that of the former deranges the functions on which its life depends. Even the structure of the lungs, we have just seen, is evidently disordered in a few hours by the division of the eighth pair of nerves in the neck.

As the function of the stomach is destroyed when the influence of the brain through the eighth pair of nerves is cut off, we should at first view infer, that it is from the brain alone that the stomach derives its nervous influence. But although the process of digestion be suspended by the division of these nerves, it does not follow that the stomach may not derive nervous influence from some other source, because the loss of any considerable part of this influence may destroy its function. Besides,

* Journal of the Royal Institution, No. 17.

† *Ib.* No. 18, page 257.

‡ *Ib.* No. 23, page 17, *et seq.*

its remaining sensibility, indicated by the efforts to vomit, proves that the influence of the nervous system is not wholly withdrawn from it by dividing these nerves.

If, then, this influence be not supplied to the stomach by the eighth pair of nerves alone, but also, as we have reason to believe from the evidence of anatomy, by nerves arising from different parts of the spinal marrow, it is evident that cutting off the influence of any considerable part of this organ, while we leave the eighth pair of nerves entire, must affect the power of the stomach, though probably not so much, because the brain, we have reason to believe, constitutes the most important part of the nervous system. The same observation applies to the lungs. On appealing to the test of experiment, such was found to be the result; the functions both of the stomach and lungs were impaired, by destroying any considerable portion of the spinal marrow, the lesion of function being proportioned to the extent and importance of the part destroyed*.

Another point relating to this part of the subject remains to be ascertained. Do the effects observed in the stomach and lungs, when part of the spinal marrow is destroyed, arise directly from the destruction of that part, that is, from the ceasing of its office, or from the influence of the brain on the spinal marrow being thus limited? It is evident, that if the former opinion be correct, the division of the spinal marrow in the middle will not produce the same effects as the destruction of the lower half. If the latter, the effects must be the same. The division of the spinal marrow in the middle produced very little deviation from the healthy state, either in the stomach or lungs, compared with that produced by the destruction of the lower half of that organ†.

Thus, it appears, that the function of the spinal marrow also is necessary to the secreting power, and that, as far as it is necessary to this power, it is independent of any influence derived from the brain. As a partial destruction of the spinal marrow impairs the secreting power, a partial abstraction of the influence

* *Inquiry*, &c. Exp. 54, 55, 56.

† Exp. 59.

of the brain has the same effect. It was found that the division of one of the eighth pair of nerves deranges the function of both the stomach and lungs, nearly in the same degree with the destruction of a certain portion of the spinal marrow*. The function of every part of the brain and spinal marrow therefore is necessary to the due performance of secretion.

Here a question of great importance in the animal economy arises. As it appears, from the experiments just referred to, that the nervous power is equally essential with the circulation of the blood, for maintaining the functions of secretion and assimilation, what are the parts they severally perform in these functions? It is evident, that the extreme parts of the sanguiferous and nervous systems are connected in a way very different from that in which these systems are connected in other parts. The heart and vessels of circulation, we have seen, can perform their function after the influence of the nervous system is withdrawn. The function of the secreting vessels immediately ceases on the interruption of this influence. We must suppose, therefore, either that the influence of the nervous system bestows on the extreme vessels the power of separating and re-combining the elementary parts of the blood, or that the vessels only convey the fluids to be operated upon by this influence.

Experiments, to which I have already referred, prove that the most minute vessels which can be seen by a powerful microscope in the web of a frog's foot, are independent of the nervous system. The motion of the blood is as rapid, and the circulation in the foot presents precisely the same appearance after as before the slow destruction of the brain and spinal marrow. If the power of the vessels of secretion had been lost by the interruption of the influence of the nervous system, would not this have necessarily occasioned some change in the distribution and motion of the blood in the web? The conclusion from these experiments is strengthened by others. In those in which the secreting power was destroyed either by the division of the eighth pair of nerves or the destruction of part of the spinal marrow, there did not necessarily appear to be a de-

* *Inquiry*, Chap. vii. Sect. 2.

fective supply of fluids. In the stomach they were often as copious, sometimes more copious, than usual; and the latter was almost always the case in the lungs. The fault seemed to be, that a due change on them had not been effected. We know that the vessels of circulation possess no powers but the elastic and the muscular, or what in many of its properties resembles the latter. Can we suppose, that the vessels of secretion, which are only a continuation of those of circulation, all at once assume a different nature; or is it at all consistent with our knowledge of the phenomena of chemistry to suppose, that by any influence the powers just mentioned, or indeed any that can be supposed to belong to vessels, could be enabled to separate and re-combine the elementary parts of the blood? The first of the above positions being set aside, it seems a necessary inference from the experiments referred to, that in the function of secretion, the vessels only convey the fluids to be operated upon by the influence of the nervous system.

It is not to be overlooked, however, that the vessels convey the fluids in a peculiar way. By the lessening capacities of the capillaries, the blood is divided, as by a fine strainer, some of its parts being too gross to enter the smaller vessels. How far the blood may thus be subdivided we cannot tell. As this structure of the vessels is uniform, we have reason to believe, that its effect on the blood is necessary to prepare it for the due action of the nervous influence.

We are now prepared to consider the question, for what purpose is the influence of the whole nervous system bestowed on the muscles of involuntary motion? Admitting, it may be said, that the due performance of secretion requires the united power of all parts of the brain and spinal marrow, and that we may therefore explain why their united influence is bestowed on secreting surfaces; the question still remains, why should their united influence be bestowed also on the muscles of involuntary motion?

It is evident, that affections of the nervous system could produce no occasional increase of the secretions, were not the sanguiferous system, and particularly the vessels of secretion,

capable of being stimulated by the same influence which operates in the formation of the secreted fluids. The increase of secreting power in any part would be in vain, were there not at the same time a corresponding increase in the supply of the fluids on which it operates. A similar observation applies to the excretory muscles, as far as they are muscles of involuntary motion. The same increase of nervous influence which occasions an increased flow of secreted fluids, excites these muscles to carry off the increased quantity. Nature does nct seem to trust this to the increase of stimulus, occasioned by the increased flow of the secreted fluid, which we have reason to believe from the *modus operandi* of certain causes of inflammation, would often occasion morbid distention. Now the vascular system, and the muscles of excretion, if in them we include the alimentary canal, comprehend all the muscles which are supplied with ganglionic nerves, unless we regard the iris as a muscle. The state of this organ is quite anomalous in the animal economy, being one of involuntary motion, excited only through the medium of the nervous system.

In the preceding view of the subject, we find the relation of the vessels of secretion to the nervous system the same as that of the heart and vessels of circulation. Their function is independent of, but capable of being influenced by, this system.

Thus, we perceive, the necessity of every part of the function which the ganglions appear to perform. A combination of the whole nervous influence is necessary to the due formation of the secreted fluids; and that there may be, under all circumstances, both a due supply of the fluids to be acted upon, and a due removal of those prepared, whether for the functions of life, or for the purpose of being thrown out of the system, it is necessary, as appears from what has just been said, that the powers which convey all these fluids should be subjected to the influence by which secretion is performed. This function, it is evident, requires a more regular supply of fluids than could have been obtained, had the usual action of the vessels depended on the nervous system, which is subject to continual variation; but had not this system been capable of influencing the vessels, not

only no change in it could have influenced the flow of secreted fluids, but every occasional increase of the influence of the nervous system, supplied to secreting surfaces, finding no increase of fluids to act upon, would necessarily have excited disease. Thus, it is requisite that the power of the sanguiferous should be independent of the nervous system, yet capable of being influenced by it; as from direct experiment, we have just seen, it is found to be.

The secreting processes are constantly attended with a temperature considerably raised above that of the surrounding medium. Does this also depend on a function of the nervous system?

It appears, from experiments above referred to, that the destruction of any considerable portion of the spinal marrow, deranges the function of secreting surfaces. Together with this effect, it was always found to lessen the temperature of the animal, more or less, according to the extent and importance of the part destroyed*. Some years previously, Mr. Brodie, in the Croonian Lecture for 1810, gave an account of experiments which led to the inference, that the maintenance of animal temperature is under the influence of the nervous system, and in the Philosophical Transactions of 1812, he relates additional experiments, tending to strengthen this inference. The experiments related in the inquiry just referred to, seem in a striking manner to confirm the opinion of Mr. Brodie. He found that poisons impairing the vigour of the nervous system, impair the temperature. It appears from my experiments, that lessening the extent of this system, by destroying part of the spinal marrow, has the same effect.

Thus, it follows, that the temperature of the animal body depends on the state of the nervous system; but many observations point out that it depends also on that of the powers of circulation. When the power of the heart and vessels is greatly impaired, so that the motion of the blood languishes, the temperature falls. If by exercise, or the use of stimulants, we increase the action of the heart and vessels, the temperature in

* *Expgr. Inquiry*, p. 161, *et seq.*, second edition.

the same proportion rises. When there is a natural defect in the organs of circulation, and particularly when this defect is such as prevents the blood passing through the lungs, with the freedom necessary to its healthy state, the temperature is found below the natural standard. The reader may consult a paper on this subject, by Mr. Earle, in the seventh volume of the Transactions of the Medico-Chirurgical Society, in which there are many excellent observations. As we proceed, we shall find proofs founded on direct experiments, that the temperature depends on the state of the circulation, and particularly on the passage of the blood through the lungs, which to detail here, would too much anticipate some of the other parts of the subject.

Whether caloric be a substance, or as some of the first chemists of our time are inclined to believe, only a certain motion of the particles of bodies, it is of course foreign to this paper to inquire; but it appears from the foregoing observations, and will, I think, appear still more strikingly from those I shall have occasion to add, that the maintenance of animal temperature must be ranked among the results of the action of the nervous system on the blood. It is on this account that I have elsewhere said, *that if caloric be regarded as a substance*, its evolution in the animal body must be ranked with the secreting processes; the definition of secretion, I conceive, being the evolution of a *tertium quid*, in consequence of that action.

When to the functions which have now been detailed, we add, that the nerves are the means of conveying impressions to and from the sensorium, we have, I believe, enumerated the whole of the functions of the nervous system properly so called.

Although it has been very generally admitted, that the nerves of the organs of sense perform no other function but that of conveying to the more central parts of the nervous system the impressions they receive, it has been supposed that the nerves of other parts, and particularly those of the viscera, are capable of so impressing each other, that these parts sympathize independently of the more central parts of the system. This position, which, were it correct, would seem in opposition to

many established laws of the nervous system, I have considered at some length in the ninety-sixth and following pages of the second edition of my Treatise on Indigestion. It appears, as far as I can judge, from the facts there adduced, that it is altogether unfounded, the nerves seeming in the latter as in the former case, only to convey impressions made on their extremities to the more central parts of the system.

(To be continued.)

Art. IV. *On Soda-Alum, in a Letter from* Dr. Ure *to* Dr. Wollaston, V. P. R. S.

Glasgow, March 1, 1822.

Dear Sir,

I now fulfil the promise which I made at my visit to London, in October last, of sending you specimens of soda-alum, a salt, concerning whose existence, or possible formation, our ingenious friend, M. Clement-Desormes, expressed, as you may remember, the strongest doubts. The larger crystals were made by Mr. William Wilson, an extensive alum-manufacturer in this neighbourhood; the smaller and more regular ones were formed by myself in the following way:

In a cold lixivium of effloresced alum schist, of specific gravity, 1.35, previously freed by crystallization from the greater part of its sulphate of iron, I dissolved with trituration in a glass mortar nearly as much muriate of soda as the liquid would take up. The solution was then left to spontaneous evaporation, in a temperature of about 60° F. At the end of two or three weeks, abundant groups of regular octohedrons were found diffused throughout the yellowish pasty magma. These crystals were washed with alcohol, and dried with blotting paper. To free them entirely from an adhering portion of copperas, they must be re-dissolved and re-crystallized.

The form and taste of this salt are exactly the same as those of common alum; but it is less hard, being easily crushed between the fingers, to which it imparts an appearance of moisture. Its specific gravity was determined by weighing in

oil of turpentine. It is 1.6 reduced to water, as unity. 110 parts of it are soluble in 100 of water at 60°; and form a solution whose specific gravity is 1.296. In these respects, potash-alum is very different; for I find that of this salt, from eight to nine parts only are soluble in 100 of water at 60°, yielding a saturated solution, the density of which is no more than 1.0465.

One hundred parts of soda-alum crystals lose by a heat verging on redness 49, which seem to be merely water; for the residuary 51 parts re-dissolve in that liquid, without causing any sensible cloudiness. By test solution of muriate of barytes, I ascertained that 100 parts of soda-alum contain 34 parts of sulphuric acid. The resulting muriates being evaporated to dryness and ignited, weighed 23.3 of which 12.3 were found by the action of water to be muriate of soda, and 11 alumina. But 12.3 of muriate of soda are equivalent to 6.56 of that alkali.

To another 100 parts of this salt, I added water of ammonia, in slight excess, filtered, and exposed the well-washed alumina to ignition, after which it weighed 10.5 parts. The filtered liquid was evaporated to dryness and ignited. It weighed 14.4 parts, and was dry sulphate of soda; for, on re-dissolving it in water, and slowly evaporating the solution, crystals in grooved six-sided prisms were obtained, amounting to somewhat more than double the weight of the ignited salt; 14.4 parts of dry sulphate of soda are equivalent to 6.4 soda. From the mean of the above results, this soda-alum seems to consist of

Sulphuric acid,	34.00
Alumina,	10.75
Soda,	6.48
Water of crystallization,	49.00
	100.23

Had leisure permitted me to repeat the analysis with greater care, it is probable that the experimental quantities might have come out more nearly as follows:

Sulphuric acid,	33.96	— 4 primes	=	20.000
Alumina,	10.82	3	=	6.375
Soda,	6.79	1	=	4.000
Water,	48.43	25	=	28.125
	100.00			58.500

For certain purposes in calico-printing, this salt seems to be well adapted, by its remarkable solubility in water. That it contains no ammonia, I ascertained, by distilling a portion of its solution, off lime.

I am, Dear Sir,

Your most faithful servant,

ANDREW URE.

ART. V. *A Translation of* REY'S *Essays on the Calcination of Metals,* &c.

[Communicated by JOHN GEORGE CHILDREN, Esq., F.R.S., &c.]

Concluded from page 141.

ESSAY XXII.

It is not the volatile salt of the charcoal that increases the weight.

As soon as I had made the rough sketch of this discourse, I sent it to the person spoken of in the last Essay, who, a few days after, put into my hands a writing, disavowing the opinion which I have there combated; and, after having opposed to my creed respecting the thickening and increasing of weight of the heated air, the reasons which I have related in the 12th Essay, and refuted in that and the two following, he proposes his own opinion briefly as follows: The increase of weight is necessarily derived, either from the vessel, the air, or the charcoal. Not from the vessel, since it loses nothing of its weight; not from the air, since heat can only subtilize and render it lighter, as he pre-supposes proved; it remains therefore, says he, that it must be the charcoal to which it is owing. And, to show how that happens, he says that charcoal contains two parts or natures, one vegetable, the other metallic; and each of these two parts, one fixed, the other volatile. The

fixed part remains at the bottom of the furnace in the state of cinder, containing the fixed salt, which is separated by washing, and the volatile part rises all round the vessel, containing in a superfluous humidity (derived from the vegetable) a volatile salt, of a metallic nature; which, raised on high on the wings of the humidity, meeting the air directly over the vessel, which is more rarefied and lighter than the vapour of the charcoal, sinks through it into the vessel and attaches itself by a close sympathy to the fixed salt of the calx of tin, which having received a certain quantity of it, and being as it were saturated, rejects the surplus; like as salt of tartar, after a certain number of cohobations, cannot be further impregnated by the volatile salt, contained in the aqua vitæ. Having run over his writing, I refuted this opinion in his presence by the following arguments. Since we must give every one credit in his own art, if we have nothing to the contrary, it is reasonable, in speaking of volatile salts, to borrow the language of the alchymists, who alone can discourse of it in a proper manner, having first discovered it and revealed it to us, when we were not aware that there was a volatile salt in nature. They found in vegetables, as in almost every thing, two sorts of salts, one fixed, the other volatile; the former containing a fixed spirit in itself, and being contained in the solid part of its subject; the latter containing a volatile spirit, and being contained in the juices. The fixed is obtained, say they, by calcination, remaining after it in the ashes. The other cannot sustain the fire, being no less volatile in effect than in name, and thus rises with the slightest heat with the juice that contains it, or is lost even by simply drying the vegetable. Now this being so, it is out of doubt, that in the whole of the charcoal there can be no volatile salt, for even the wood of which it is made cannot contain any, since it is previously dried; and even if it did contain any, who but sees it must of necessity lose it, when in the charring it becomes a burning coal? Truly, were I to grant this favour, and allow that charcoal contains volatile salt, they who know how rare it is in every thing, will never believe that from the small quantity of charcoal which the Sieur Brun used

in his calcination, so great a quantity should have proceeded. For we must not only suppose seven ounces, but also that which has replaced the diminution of weight derived from the loss of vapours of the tin, and from its increased volume, and moreover, all that the fumes of the charcoal have carried off elsewhere, not only through the whole laboratory, but out of it, whither they have escaped in abundance by the openings. In which fumes, if there were salt in proportion to that which has sunk into the vessel, from those fumes that were above it, and it were all collected together, truly the collection would be monstrous!

And besides, when the calx of tin had taken its fill of salt, by this imaginary sympathy, what prevents the continuation of the fire from accumulating more above it, and filling the vessel, since it descends into it by its own weight? Experience refutes all this—besides that I have demonstrated, that the air above the vessel is so thick, that it could not descend into it. Moreover, if a furnace be constructed in a wall separating two chambers, so that the vessel may be on one side, and the registers and doors for putting in the charcoal and admitting air on the other, I maintain that the increase of weight will ensue, though no vapours can enter the chamber containing the vessel. This I have confirmed by proof, which I made in the forges of my elder brother, John Rey, Sieur de la Perotasse; when I found an equal increase in tin that I calcined, on what they call a *gueuse,* or ingot of iron, weighing from sixteen to twenty quintals, at the instant that flowing from the furnace it is thrown into its mould. Now it cannot be said, that the vapours of the charcoal contribute any thing in this case. Wherefore this volatile salt is inadmissible in this matter.

ESSAY XXIII.

Volatile mercurial salt is not the cause of the increase.

Some days after the refutation I have just mentioned, the same person sent me another of his opinions; namely, that the volatile salt is of a mercurial nature, not containing any one of the three principles absolutely pure, but mixed with the others, in

such manner that the true fixed salt exists in the volatile salt, as well as another less earthy of a sulphureous nature, and a third, greatly subtile and penetrating, partaking of the nature of mercury. Now crude and cold mercury readily penetrates through gold, and attaches itself closely to it within and without; so that it is not unreasonable to imagine, that the volatile salt of a mercurial nature, rarefied by and rendered much more penetrating by fire, should pass through the substance of the vessels, which are in like manner heated and rendered more penetrable by the fire, and should attach itself to the calx of tin, by a certain sympathy that may exist between them, as well as between gold and crude mercury. This second opinion is sufficiently overturned by the reasoning in the preceding essay; for having shewn that there can be no volatile salt in charcoal, who but must see that it cannot exist in any manner whatever? On the other hand, if this mercurial salt penetrated the vessels, it would necessarily dissolve them and make an amalgam with them, which by no means happens in our calcination. Besides, since crude mercury exhales, as we see, at a very low heat, how can it happen that this mercurial salt, being of so subtile a nature, after it has penetrated the vessel, should remain in the burning calx, without quickly flying off? Moreover, if we are to find in each of the principles all the three, I do not see why each of these again should not contain them all, and so on *ad infinitum.* These speculations are manifestly subtile, but have no foundation in nature.

ESSAY XXIV.

It is not moisture attracted by the calx that increases its weight.

Conversing, not long since, with a learned man of good judgment, I took the opportunity of mentioning this subject to him, on which, not having thought at all on the matter, he told me, that he believed that the calx, by its great dryness, attracts much moisture, and that it is this which increases its weight. I cannot approve of this opinion, for the following reasons: First, because I have never learnt that a contrary attracts its contrary; it rather flies from it, or drives it away if it can. Be-

sides, by moisture cannot be understood a naked quality, but rather water or air invested with it. As to water, whence could the calx derive it, none being near it? Is there any air more humid than usual in the laboratory in which the calcination is carried on? Will not the heat of the furnace have absorbed it? And if the calx should attract so much water, or cloudy air, as to increase its weight, a fifth part or thereabouts, as by the experiment, we should have a mortar rather than a dry calx. I add, besides, that at the instant of calcination the calx will be found increased before it can have had time to affect this imaginary attraction.

ESSAY XXV.

By one single experiment, all the opinions opposed to mine are wholly destroyed.

It is said of Hercules, that he had no sooner cut off one head of the hydra that ravaged the *Lernean marsh*, than two sprung forth—my state is like his. The error I combat abounds in opinions, which are so many heads; if I cut off one, two start up: my labour is always increasing, and I believe I shall never have done, if I employ myself in chopping them off one after another. I must collect my strength, and stiffen my arm, to sever them all at one blow, and lay the monster low in death. Let him beware, whom it concerns, for here I deal the deadly stroke. I have read in Hamerus Poppius, in the third chapter of his book, entitled "Basilica Antimonii," his new method of calcining antimony. He takes a certain quantity of it, weighs it, and, having reduced it to powder, places it in a conical form on a marble slab; then, placing a burning-glass in the sun's rays, he directs their focus on the apex of the cone of antimony, which then fumes abundantly, and in a short time the part on which the rays fall is converted into a very white calx, which he separates with a knife, and directs the rays on the remainder, till the whole becomes white, when the calcination is complete. It is a wonderful thing (he adds afterwards), that although in this calcination the antimony loses much of its substance, by the vapours and fumes which exhale copiously, yet, so it is, its

weight increases instead of diminishing. Now if we demand the cause of this increase, will Cardon tell us it is the disappearance of the celestial heat? rather it is more largely infused, by means of the solar rays. Will Scaliger say it is the consumption of its aërial parts? the particles of the calx being much attenuated and its volume increased, it rather *absorbs* them more abundantly. Will Cæsalpine allege his soot as the cause? there is no fire in this case to produce it. Can the vessel have contributed any thing towards it? the rays of the sun are so dexterously directed on the antimony, that they never touch the marble. Will any one suggest the vapours of the charcoal? none is employed. As to the volatile salts, that have been so ingeniously brought forward, they entirely lose, in this case, their savour and their grace. Peradventure moisture will be adduced, as some one very lately has thought fit to do. Whence comes it? From the marble? No, that is not conceivable. From the air? Still less so: for this operation is best performed in the hottest days of summer—in the most violent heat of the dog days, when every thing is so heated here below, that even in the shade, or during the night, the air dries wet linen, and the damp ground; and in the day-time, where the sun falls, it tans our skin, withers the grass, burns up the fruits, dries the woods, soaks up the lakes, lowers the streams of the rivers, and inflames combustible substances, as a heap of pigeons' dung. To look for humidity in the air to moisten our calx and give it weight, at that season, not by night, but by day, not in the shade but in the sunshine, not when the sun simply shines, but when its rays, collected in a concave mirror, are reflected with so much violence that they fuse and calcine metals; to look for moisture under such circumstances, I say, is to seek fire in ice, and a knot in a bulrush, as we say, a thing never to be found. Now let all the ablest minds in the world be melted into one, and this fine spirit exert itself to the very utmost of its strength, let it seek attentively on the earth and in the heavens, let it rummage all the windings and turnings of nature, yet will it not find the cause of this increase, but in the air alone, which heated, thickened and made

heavy by the sun's rays, mixes with the calx in proportion as the antimony is attenuated by the calcination, and adheres to its minutest parts. This fact fully establishes the truth of my opinion as to the increased weight of the lead and tin, which can have no other cause than the mixture of the thickened air—there being no difference between their increase and that of the antimony, except that in the latter case the air is thickened by the solar rays, and in the former by the heat of common fire.

Essay XXVI.

Why the calx does not increase in weight ad infinitum.

Having thus confuted the opposite opinions, my own alone can freely keep the field. It is true, that perceiving some objections which might disturb its progress, I step forward to set them aside. The first would seem to lead to the absurdity I have objected to Cæsalpin, that, admitting my opinion, the calx will increase *ad infinitum.* For why it will be said, will not the calx increase infinitely, since the fire may be infinitely continued, which will always supply that thickened and heavy air which causes its increase? I rid myself of this difficulty, which might insnare one of the less subtle sort, by observing, that all matter which increases by the addition of another is either solid or liquid, and that the mixture takes place between them in three ways. Either the solid matter mixes with solid, or the liquid with liquid, or one of them with the other. The mixture and increase which is effected in the two first ways has no bounds. You may mix sand with sand for ever, the increase will have no end. So you may mix wine with wine, and never have done—but it is not so with the third method, when we add and mix together a liquid with a solid: such mixed addition will not increase for ever, will not go on to infinity. Nature, in her inscrutable wisdom, has here placed bounds which she never oversteps—mix water with sand or flour, their smallest parts will be enveloped by it; pour a fresh quantity on, and they will not take up any more; and if we withdraw them from the water, they will only carry off what adheres to them, and is sufficient exactly to encompass them. Immerse them

again a hundred and a hundred times, they will not come out more loaded with it, and if left at rest in the water, they quit the superfluous quantity, and sink of their own accord to the bottom: so religiously does nature stop at the limits she has once prescribed to herself. Our calx is in this predicament; the thickened air attaches itself to it, and goes on adhering by degrees to its most minute particles: thus its weight increases from the beginning to the end; but when the whole is clothed with it, it can take no more. Do not continue your calcination with that hope; you will lose your pains. As for the rest, let not what was said in the eleventh chapter disturb you, where I used the expression, "this air, no longer air, but rather an air deprived of its nature," for these are words in excess, by which I mean only, that this air has been deprived of that liquid subtlety, which prevented its adhering to any thing, and is rendered more gross, heavy, and adhesive.

Essay XXVII.

Why all other calces and ashes do not increase in weight.

I proceed to another objection that may be started. Why do not all other calces and ashes, formed by the force of fire, increase in weight as well as the calx of tin or lead? What privilege have these more than the rest? I answer, that those things which are calcinable, or convertible into ashes, are of different natures. Some have a great deal of exhalable and evaporable matter; or (speaking like the alchymists) much sulphur and mercury, which the fire continually drives off to the end. Hence, much loss, few ashes, which cannot attach to themselves so much of the thickened air, as even to supply the loss. Others have little exhalable or evaporable matter, or in other words, little sulphur and mercury; hence small loss, much ashes (for the abundance of salt) which attracts so much of the thickened air, that not only the loss is supplied, but besides the weight increases largely beyond it. Stones, vegetables, and animals commonly are of the first order; lead and tin, of the second. There are other things whose volume is so much extended by calcination, that even if little or no matter

were lost, their weights would nevertheless greatly diminish, not so much when examined by reason, as by the balance. Such are the Indian metal called *calaem**, and some kind of saffron of Mars, as the chemists observe.

Essay XXVIII.

If Lead increase in weight like Tin.

I should have made an end, but the Sieur Brun writes to me, that having observed the increase of the tin, he made the same experiment on lead, which he found lost an ounce in the pound; which plunged him further in doubt, expecting to have found the same increase as in the tin, from the similarity of their nature, and having been calcined by the same process. But to the Sieur Brun's experiment, I oppose those of Cardan, Scaliger, and especially Cæsalpin, already brought forward, who says that it is worthy of admiration, that common lead when calcined, increases in weight eight or ten pounds per cent. Shall I array these persons in the debate, each in support of his own experiment? I am too peaceable—I reconcile them thus. One kind of lead is purer than another, whether from its coming from different mines, or from its having been melted before. The above named found an increase with the purest; the Sieur Brun a loss in the other.

Conclusion.

Behold, now the truth, whose brilliancy dazzles your eyes, which I have dragged from the deepest dungeons of obscurity, whose approach has hitherto been inaccessible. It is she who has made all the learned men sweat with vexation, who, desirous of becoming acquainted with her, have been compelled to leap over the difficulties that surrounded her. Cardan, Scaliger, Fuchsius, Cæsalpin, Libavius have sought her curiously, but never found her. Others may seek her in vain, if they follow not the road which I have first cleared for them, and made royal; all the rest being only thorny paths, and inextricable labyrinths that never lead to an end. The labour has been mine—to the reader be the profit of it, to God only the glory.—END.

* *Zinc.*

ART. VI. *On the Separation of the Proximate Principles of Animal Substances.* *By* W. T. BRANDE, Sec. R.S., and Prof. Chem. R.I.

IN certain analyses of animal products, as for instance in ascertaining the relative proportions of albumen, gelatine and water, in the different kinds of muscular fibre, it is frequently difficult to deprive the substances under examination of humidity, without at the same time occasioning a partial decomposition of the dry residue. Under these circumstances I have availed myself with considerable advantage of the air-pump vacuum, including a surface of sulphuric acid, and have been so much struck with the rapidity and perfection of the process in these cases, as to be induced to recommend it as a very satisfactory and easy mode of depriving organic bodies in general of their adhering moisture. I have for some time contemplated a series of comparative experiments upon the varieties of muscular fibre employed as food, and one material object in these inquiries was to determine the actual quantity of nutritive matter contained in given weights of such products: as far as this has been satisfactorily effected, I shall state the results, principally as illustrating the advantage of the method above mentioned of depriving the substances of water.

500 grains of recently killed veal, free from fat, and cut into small shreds, was exposed upon a metal plate to a temperature of 212° until it ceased to lose weight, when it was found to have sustained a loss = 375 grains.

A similar portion of the same fibre, submitted to distillation in a retort at the temperature of 212°, could not be so perfectly dried, but the water which passed over had a peculiar smell, and was rendered slightly turbid by solutions of nitrate of silver, subacetate of lead, and corrosive sublimate: these solutions were rendered much more turbid by the water which passed off when the heat of the retort was elevated to 320°; so that it would appear that although rapid desiccation at 212° occasions but a slight change, exclusive of the separation of water, a partial decomposition ensues at temperatures between

212° and 320° in which the water carries over an appretiable portion of animal matter, detected not only by the smell, but by the action of the above-mentioned metallic solutions.

500 grains of the same veal were placed in a small glass basin under the receiver of an air-pump, including a circular surface of sulphuric acid of one foot diameter; during the exhaustion the meat increased in bulk from the evolution of air bubbles, but soon shrunk again, and in two hours appeared perfectly dry; it was removed, and being weighed was found to have lost 370 grains, so that the loss fell short of that occasioned by desiccation in the atmosphere at 212° by 5 grains only, or 1 per cent. These comparative experiments were frequently repeated with similar results, the desiccation at low temperatures in vacuo always proving nearly equal to that at high temperatures in the air; and being effected without any risk of decomposition, the former process was preferred to the latter.

To determine the relative proportions of gelatine and albumen in the dried fibre, it was coarsely powdered and digested in repeated portions of warm and ultimately of boiling distilled water until the residue was no longer acted upon; it was then collected upon a filter, and dried as before; the loss of weight indicated that of the matter soluble in boiling water, and which consisted almost entirely of gelatine: a very small portion of fat was sometimes observed upon some of these solutions, but where care was taken to operate upon perfectly lean muscular fibre no fat was detected in the water employed for separating the gelatinous portion.

The results of a few of these experiments are thrown together in the following table; they show the quantity of real nutritive matter contained in 100 parts of different fibres, and which, when care was taken to exclude adhering substances, was found to vary very little in the different muscles; they also show the proportion of nutritive matter soluble in water, and removed by the process of boiling at 212°.

100 parts of Muscle of	Water	Albumen or fibrine	Gelatine	Nutritive matter
Beef	74	20	6	26
Veal	75	19	6	25
Mutton	71	22	7	29

100 parts of Muscle of	Water	Albumen or fibrine	Gelatine	Nutritive matter
Pork	76	19	5	24
Chicken	73	20	7	27
Cod	79	14	7	21
Haddock	82	13	5	18
Sole	79	15	6	21

It appears evident from the above table, that although in the different kinds of meats and fish there is a slight variation in the nature and proportion of nutritive matter, it is not such as to account for the different facility with which they are digested and dissolved in the stomach: this probably depends upon some peculiarity in the solubility of the albuminous portion, and which it is my intention more particularly to examine.

The large proportion of water which is contained in muscular fibre, and the great loss of weight which it consequently suffers by desiccation I long ago observed, and was inclined to attribute it to a partial decomposition of the animal matter; but the close approximation of the results obtained by exposure to heat with those afforded by drying in vacuo, aided by the absorbent powers of sulphuric acid, show that no such decomposition occurs, and that the loss of weight sustained by muscular fibre, dried at 212°, may be referred without appretiable error, to the evaporation of water only.

Art. VII. *An Account of the Periodical Literary Journals which were published in Great Britain and Ireland, from the year* 1681, *to the middle of the Eighteenth Century. By* Samuel Parkes, F.L.S. M.R.I. *Soc. Amer. Socius., &c. &c.*

[Concluded from Page 58.]

The History of the British Periodical Literary Journals, as far as it has been written, comprises Twenty distinct articles, and brings the narrative down to the year 1720. The following is an account of the subsequent works, in the order of time in which they were published.

XXI. 1722. "Bibliotheca Literaria, being a Collection of Inscriptions, Medals, Dissertations, &c." Printed for W. and J. Innys, at the West End of Saint Paul's. London.

The editor, who was the learned Dr. Samuel Jebb, has prefixed an introduction to the first number, which I shall copy for the sake of shewing the nature of the publication.

"The undertaker of this work having observed that many things, which are useful and valuable, are in a manner lost to the world, by reason of their being too small to be separately published, has judged it will be acceptable to the learned and curious, to have 'em collected into one body, and sent abroad as occasion shall require.

"The particulars which fall within the bounds of his design are,

"I. Inscriptions and Medals, which are discovered from time to time, and cannot be printed apart, and must therefore lye concealed from the public, unless made known in this manner.

"II. The small Tracts of ancient and approved writers, which lye dispersed in libraries, and have not yet been published.

"III. Critical Dissertations upon Authors or Things.

"IV. Whatever tends to the explaining any part of Antiquity, Notes upon the Fathers or Classicks, Emendations of corrupted Passages, together with the readings of Manuscripts not already collated."

"Of these Collections he proposes to send abroad about six or seven sheets once in two months. At the close of each performance will be added an account of the labours of the learned; what works of value are preparing for the press abroad, or at home in our own universities."

The above extract will sufficiently shew the nature of this periodical work; the contributors to which were Dr. Samuel Jebb, the editor; the eminent John Masson, who was tutor in the family of Bishop Burnet; Dr. Brett, of Queen's College, Cambridge; the Rev. Joseph Wasse, rector of Aynhoe in Northamptonshire, who was the most learned man of his time;

Dr. William Wotton; Dr. Jortin; Mr. Samuel Barker, and others. The 1st and 2d numbers were published in the year 1722; the 3d, 4th, 5th, and 6th, in 1723, and the remainder in 1724. Only ten numbers of the work were published. These are usually bound in one quarto volume, consisting of about 500 pages. I consider this to be one of the most valuable Literary Journals I have; and in consequence of the opinion which the public have of it, it has long been a very scarce and dear book.

XXII. 1725. "New Memoirs of Literature." The first number of this work appeared in January 1725, and the whole was completed in six volumes 8vo. The full title runs thus, —"New Memoirs of Literature, containing an Account of New Books, printed both at home and abroad, with Dissertations upon several Subjects, Miscellaneous Observations, &c." Printed for William and John Innys. The advertisement prefixed to the first volume is short and yet sufficiently to the purpose.

"Though I had no inclination to go on with my *Memoirs of Literature*, yet I have resumed them at the desire of Messieurs Innys. I shall only observe that I have been desired to take from the Foreign Journals what I think proper to be communicated to the English readers, in order to make this new Journal more universal; and that whenever I publish an extract, of which I am only the translator, I shall give notice of it."

April 12*th*, 1725. Michael de la Roche.

These memoirs were published monthly, without interruption, for three years. Six numbers made a volume, and each volume was concluded by a copious index. The last volume was completed at the end of the year 1727, when the work was discontinued in consequence of Messrs. Innys having been engaged by Mr. Andrew Reid to publish a new periodical Journal under the title of "The History of the Republic of Letters." In justice, however, to the memory of Mr. de la Roche, it must be said that the New Memoirs of Literature appear to have been

conducted with the same judgment and spirit as the former work: and that the Memoirs of Literature, and the New Memoirs, making together 14 volumes, are books which every literary man ought to possess.

The first series of eight volumes contains 597 articles, many of which are reviewed at considerable length; and not a few of them are works of great curiosity and interest. The latter six volumes (the New Memoirs) comprise 348 articles, besides the account of Books newly printed in the different countries of Europe. Each of these volumes is closed by a well arranged and copious index.

In my account of the first series of the "Memoirs of Literature," I have hinted at the valuable information which is to be derived from those volumes respecting the life and writings of Michael Servetus; it will therefore be proper to apprize the public that many other particulars respecting that great man, were afterwards collected by Mons. de la Roche, and that these Biographical Notices are to be found in the first and sixth volume of the *New* Memoirs of Literature; particularly "An Account of a very scarce edition of Ptolemy's Geography, published by Michael Servetus, in the year 1535." This edition was enriched by Servetus, with 50 maps, "each map, accompanied by an historical description, giving an account of the country and its inhabitants. These descriptions," says La Roche, "seem to be written with a good taste and judgment. The style is clear and concise, and does honour to the author." Some of the accusations, produced by Calvin against Servetus on his trial, were founded upon his publication of this work. If I am not travelling too far out of the path which I had prescribed to myself, I am desirous of observing that Servetus was likewise the editor of an edition of Pagninus's Bible, which he published at Lyons in 1542, with a preface of his own, for which the Bookseller paid him Five Hundred Livres. Both these works are now extremely rare, but the curious inquirer who may happen to be at Paris, will, perhaps, thank me for informing him that both of them may be found in the library of St. Genevieve.

XXIII. 1728. The "HISTORY OF THE PRESENT STATE OF THE REPUBLIC OF LETTERS." This very valuable work which was edited by Andrew Reid, commenced in January, 1728, and was completed in 18 vols. Octavo, in December 1736. It is upon the plan of other Journals, and contains an entertaining and instructive review of some of the best books in the English language, besides occasional papers of value from the works of foreigners. Of this class, a Translation of the Euloge upon Sir Isaac Newton, pronounced by Fontenelle before the Royal Academy of Sciences at Paris, and which is given in the 1st vol., may be taken as a favourable specimen. Each volume contains an analysis of, and extracts from, about 40 distinct publications of the time, on various branches of literature and science. The title-pages bear the following motto from Horace:

> ——— Fungar vice cotis, acutum
> Reddere quæ ferrum valet, exsors ipsa secandi.

And from the manner in which the work was conducted, the editor appears to me to have fully redeemed the pledge which this motto continued, month after month, to offer to the public. The first volume commences with a sensible preface, highly deserving the attention of every editor of a Literary Journal; but it is too long to be transcribed for my present purpose. The motto which the author has chosen for this preface is

> "Tros Rutulusve fuat, nullo discrimine habebo."

A work of this extent, and containing such variety of matter, ought to have had a general index. In this it is deficient; but each number contains a table of contents, and each volume closes with a very complete index to that particular volume. It is not very often that a complete set of the "Present State of the Republick of Letters" is offered for sale, but the work is certainly not so scarce as some of the more early literary journals; and detached volumes are so often to be met with, that an industrious collector might perfect a set without much difficulty, and at a trifling expense.

XXIV. 1730. " A LITERARY JOURNAL, *or a Continuation of the Memoirs of Literature.* By the same Author." (Meaning Mons. de la Roche.) The first volume commences in January, and concludes with the number for June, 1730. Vol. II. from July to December, 1730, octavo. They were sold by Knaplock, at the Bishop's Head, in St. Paul's Church Yard.

On the back of the title-page of the last number of the New Memoirs of Literature, M. de la Roche announced that he should discontinue writing those Memoirs ; and on the same page he says, " When I publish another English Journal with another title, I shall give notice of it."

Accordingly, two years afterwards, this learned and indefatigable man began a new work, under the title which stands at the head of this article, and from the attention which I have been able to pay to this performance, it appears to me to have been conducted with his usual care and discrimination. I have seen only the second volume, but I can pronounce that this abounds with curious matter, as well as a regular review of many very important works. This Journal was published quarterly, and at the back of the table of contents of the number for the last quarter of the year, the following advertisement appears :

" This part of the Journal would have come out at Christmass if I had not been sick. I shall always use my utmost endeavors to publish these Papers in due time. As I print this work upon my own account, I hope my Readers will do me the favour to recommend it to their friends." That M. de la Roche printed this work *on his own account,* may be readily believed, as it was got up in a style far superior to any English Journal that had then appeared. The paper is fine, the types are very similar to Baskerville's, and the manner in which it is printed would not disgrace the proudest Printer in our Metropolis at the present day.

XXV. 1730. " HISTORIA LITTERARIA," or an exact and

early account of the most valuable books published in the several parts of Europe, with a complete alphabetical index."

> "*Floriferis ut apes in saltibus omnia libant,*
> *Omnia nos itidem.*————— Lucret.

London, Printed for N. Prevost, over against Southampton street, in the Strand. 1731.

This Journal, which was published in monthly numbers at one shilling each, commenced in the year 1730, and closed in 1733, or early in the year 1734, forming four volumes in octavo. It was written by the noted Archibald Bower, formerly Professor of Rhetoric, History, and Philosophy, in the Universities of Rome, Fermo, and Macerata, and author of the History of the Popes, in seven volumes, quarto. It was undertaken and completed by him while he resided with Lord Aylmer. With this nobleman he lived on terms of the greatest intimacy, and was introduced by him to a large circle of literary acquaintance.

It is a curious circumstance that he (Archibald Bower) wrote the preface, and several of the first articles, in Italian, because he did not sufficiently understand English to allow him to write in our language; but finding the inconvenience and expense of employing a translator, he applied so closely to the study of the English tongue, that in six months he was enabled to dismiss his translators *, and continue the work himself in English †.

The following extracts from the Preface will shew the plan and design of the work:

I. " We propose to take notice of none but the most valuable books, and such as are *last* published, and have not been mentioned by any other of our Journalists. Of these, whether Latin, Italian, French, English, *&c.*, we shall give faithful extracts. The choice and novelty of books is what chiefly recommends a Journal; and the correspondents we have already settled is such, that we may confidently affirm, no work of any figure or

* The several articles which he wrote in Italian were translated by Mr. Barkley, who was the master of an academy of some note at Little Chelsea; and the preface was translated by Mr. Lockman.

† Chalmers, vol. VI. page 257.

reputation, will be published in any part of Europe, but we shall immediately give an account of it. The extracts we shall make will enable such persons as have not much time upon their hands, to treasure up a great number of excellent observations, in the various branches of literature; and at the same time acquaint them with whatever is worthy observation in the works of those writers who are the ornament of the age."

II. "Our Journal will include all subjects; nor will Dissertations on Medals, Inscriptions, and other valuable remains of antiquity be omitted."

"This Journal will always consist of Five sheets and a half, and be published regularly at the beginning of every month, Six numbers will make a volume, to which will be annexed a Catalogue of the Authors from whom the Extracts are made, together with an Index. At the end of each Journal will be inserted the freshest accounts of all Works just published, and of all Old Authors reprinted in any part of Europe, as soon as our Correspondents shall transmit us such accounts. And lastly, we shall add a Catalogue of New Books imported monthly."

As an Editor, Mr. Bower was indefatigable; and the manner in which he executed the HISTORIA LITERARIA exalted his reputation considerably. His intimacy with Lord Lyttelton and other persons of eminent learning, together with his residence in the family of Lord Aylmer, occasioned him to be still more known to the public; and while busily employed in conducting the Historia Literaria the Proprietors of the Universal History applied to him to engage him in that undertaking *. Some very advantageous offers were made to him, but so attached was he to the employment in which he was then occupied and so desirous to render the Historia Literaria deserving of the patronage of the public, that he declined listening to their proposals until the year 1734, when the publication of the Historia Literaria was relinquished. It may be added that in the following year, Mr. Bower did agree with the proprietors of the Universal

* Chalmers, Vol. VI. page 258.

History, and continued to be one of the editors of that voluminous work, for nine years afterwards.

XXVI. 1733. "THE BEE, or Universal WEEKLY Pamphlet, by a Society of Gentlemen and Booksellers, containing, among other things, an account of the most remarkable Books published abroad, and a catalogue of all books and pamphlets published at home in the week, with short reflexions upon such as deserve it." Each number consists of 44 pages, price 6d, and the Title page of each contains a wood-cut of a Bee-hive in the midst of a huge heap of books of all kinds and sizes. This work, which is complete in 7 Vols. Octavo, was planned and conducted by the unfortunate Eustace Budgell, son of Dr. Gilbert Budgell, and grandson of Dr. Gulston, Bishop of Bristol. This elegant author was early in life engaged with Addison, his intimate friend and relative, in writing papers for the Spectator and Guardian. Those marked with an asterisk were written by Mr. Budgell. The first weekly number of the Bee was published in February 1733, and the last number on the 30th November 1734. Thirteen numbers formed a volume, so that a volume was completed every three months. The life of the editor of this work was chequered with various adventures, and his death was of the most unfortunate and melancholy kind. A particular account of his history may be seen in Chalmers, Vol. VII. page 246—251.

XXVII. 1735. "THE LITERARY MAGAZINE, or the History of the Works of the Learned, containing an account of the most valuable Books published both at Home and Abroad, in most of the Languages in Europe, and in all Arts and Sciences; with proper Observations on each Author. To which are occasionally added, Biographical Memoirs, Dissertations, and Critical Enquiries. By a Society of Gentlemen." The title of this Journal which I have copied entire, will be sufficient to shew its nature and objects. One of the principal editors was Ephraim Chambers, the Author of the English Cyclopædia in 2 vols. folio. The Literary Magazine was published monthly in

Octavo Numbers, commencing in January 1735, and 12 numbers formed a volume. The second volume ends with December 1736; and I have reason to think more volumes were published. There are two Indexes to each of the volumes I have; the first being a list of books reviewed; the last an alphabetical reference to all the miscellaneous matter contained in it.

In the preface, Mr. Chambers carries the antiquity of Literary Journals much higher than any writer with whom I am acquainted.

"A history of the works of the Learned," says he, "is far from being a new project. Apollodorus an Athenian, who lived about two hundred and forty years before the Nativity of Christ, composed a work of this sort, which he called, *a Library of the Origin of the Gods;* that is, a collection of the most ancient histories, as they lie disguised under fables and fictions. We have still three books of it. Diodorus of Sicily, in the reign of Augustus, spent about thirty years in composing an *Historical Library*, in forty books, of which fifteen are now extant. But the richest and most comprehensive work of this sort is the *Myriobiblia*, written by *Photius*, patriarch of Constantinople, in the middle of the ninth century of the christian æra. It is usually called the Library of Photius, and contains the arguments or extracts of nearly two hundred and eighty volumes of different authors, on several subjects *. These laborious and useful works have preserved to us some valuable fragments of antiquity, which would otherwise never have come to our knowledge."

* This Bibliotheca was composed by Photius while he was an Ambassador in Assyria. It seems our editor did not overrate this work, for Fabricius calls it, "*non liber sed insignis thesaurus,* not a book, but an illustrious treasure," in which are contained many curious things no-where else to be found. David Hoeschelius first caused it to be printed in 1601; in Greek, at Vienna. Schottus translated it into Latin, and afterwards the Greek text and the translation were printed together at Geneva in 1611, but the best edition is that printed at Rouen in 1653, folio, under the title "Photii Myriobiblion," &c. There are large-paper-copies of this edition, which bear a very high price. See Chalmers Vol. XXIV. page 473.

In the conclusion of the preface we find the following judicious observations. " The spirit of partiality is a dangerous rock, on which many Journalists have been lost. We conceive it to be the duty of a Journalist to give a faithful account of the books which come into his hands. When he affects the air and language of a censor, he invades the undoubted right of the Public, which is the only sovereign judge of the reputation of an Author, and the merit of his compositions. To the same judge we must submit the present performance; with a fix'd resolution of neither offending those from whom we may chance to differ in opinion, nor misrepresenting, nor disguising the sentiments of authors, when contrary to our own. We shall be particularly careful to insert nothing, which has not some tendency to improve the mind, form the judgment, or entertain the reasonable and commendable curiosity, of our readers."

XXVIII. 1737. The " British Librarian : Exhibiting a compendious Review or Abstract of our most scarce, useful, and valuable Books in all Sciences, as well in Manuscript as in Print: with many Characters, Historical and Critical, of the Authors, their Antagonists, *&c.* In a manner never before attempted, and useful to all Readers. With a complete Index to the volume. *Multa renascentur quæ jam cecidere.*" London, printed for T. Osborne, in Gray's-Inn, 1738.

Although this was an anonymous publication it is well known that the Author of it was the celebrated Bibliographer William Oldys, Librarian to Robert Harley, Earl of Oxford, and one of the compilers of the Harleian Miscellany. As only six numbers of this Journal were ever published, the work is complete in one volume. For several years, this curious Journal was entirely neglected by the Book Collectors, but it is now valued as it deserves. It is a thin volume of 418 pages, and in Triphook's Catalogue for 1816 it stands valued at a Guinea, but since that time a copy was sold at Mr. Evans's sale in Pall-Mall for 1*l.* 11*s.* 6*d.*

It is observable that the British Librarian is chiefly a review of very scarce old books, and it is highly valuable because

it contains many curious details from interesting books, which are no where else at this time to be found. The situation which Mr. Oldys held with Lord Oxford gave him the best opportunity for gratifying his taste for ancient and curious books and manuscripts, and hi industry was such, that he made the most of his advantages. He is well known to have been a man of the strictest integrity—therefore whatever he asserts as of his own knowledge, in the British Librarian or in his other works, may be depended upon. An instance of his probity occurred soon after the publication of his Life of Sir Walter Raleigh, that was prefixed to the Folio edition of Sir Walter's History of the world. Some Booksellers thinking his name would tend to sell a work they were publishing, offered him a considerable sum of money if he would allow them to affix it—but he rejected the proposal with the greatest indignation, though at the time he was in great pecuniary distress.

Mr. Oldys was a writer on a great variety of subjects, and several of his MS. volumes are preserved in the British Museum. Mr. Chalmers has made out the most complete list of his works that he was able to compile, amounting to 24 articles*—from whence it appears that he was the author of several Lives in the Biographia Britannica, bearing the signature G. the initial letter of Gray's-Inn, where he formerly lived. He was also a writer in the General Dictionary, and furnished several communications to Mrs. Cooper, for that curious volume which she published under the title of the "Muses Library." A translation of Camden's Britannia, in two volumes quarto, has also been ascribed to him by Mr. Gough; and among the Birch MSS. in the British Museum, are some Memoirs of the family of Oldys, drawn up by William Oldys himself.

The following well-turned anagram was found in one of his MSS.

W. O.

In word and Will I AM a friend to you,
And one friend OLD IS worth a hundred new.

I should have been glad to give a list of the books which Mr.

* General Biog. Dict. Vol. XXIII. p. 336.

Oldys has reviewed in the British Librarian, as it could not fail to create a desire in many readers to possess the volume, but I thought the catalogue might be deemed too long for the Journal of Science, Literature, and the Arts. There are few books whose value might not be enhanced by a good Index: the British Librarian, which requires such an appendage as much as most books, is terminated by an alphabetical Index of 52 columns closely printed.

XXIX. 1737. "THE HISTORY OF THE WORKS OF THE LEARNED, giving a general view of the state of Learning throughout Europe, and containing an impartial account and accurate Abstracts of the most valuable Books published in Great Britain and Foreign parts."

"Interspersed with Dissertations on several curious and entertaining subjects, critical Reflections, and Memoirs of the most eminent Writers in all branches of polite Literature. London, printed for T. Cooper, at the Globe in Paternoster-Row, *&c.* 1737."

This work commenced in January 1737, and was regularly published in Monthly Numbers, six of which formed a volume; but there was no regular series of progressive numbers, as is usual in works of this nature, the volumes in each year being marked I and II. This Journal met with so much encouragement that it was published without intermission until the end of the year 1743, when the last Number was printed, which completed the fourteenth volume. The plan of the Editor is sufficiently explained in a copious Preface prefixed to the first volume, but I have not been able to ascertain by whom the work was written or edited. Each volume contains a review of about 40 or 50 distinct articles, and an alphabetical Index to every six numbers adds very much to the value of the work.

XXX. 1744. "A LITERARY JOURNAL." The following is a copy of the Title pages of the first Volume. "A Literary Journal for October, November, December, 1744. Vol. I. Part I. Dublin, Printed by S. Powell, for the Author, 1744." "A

Literary Journal for January, February, March, 1745. Vol. I. Part II. Dublin printed by S. Powell for the Author 1745." As this is the only Literary Journal that I am acquainted with, which was published in Ireland, during the whole of the period from the printing of the first British Journal in the year 1681 to the middle of the last Century; and as the work has for many years been very rare in England, I conceive that those persons who can read this article with any degree of interest, will be glad to have a more particular account of this Periodical Dublin Journal, than has been given of some of the works that preceded it.

The principal design of the Editor of the Dublin Journal was to give an account of the best Foreign books, published in the various languages of Europe; with such extracts from each as were most likely to interest the English reader. It commenced at the latter end of the year 1744, and was intended to be published quarterly, at the price of one shilling and sixpence for each part. The work is now complete in Five volumes Octavo, two Quarterly Numbers forming each of the three first volumes, as is the case with the "Quarterly Journal of Science, Literature and the Arts," now edited at the Royal Institution of Great Britain; but on account of a delay in the publication which was occasioned by the ill state of health of the editor, the fourth volume is made to consist of two half yearly numbers from March 1746 to March 1747; and in consequence of continued illness the 1st part of Vol. V. extends from March 1747 to March 1748, and the last number includes the literature of Europe from March 1748 to the middle of the year 1749.

From the manner in which the editor of the Dublin Journal executed his task, the work must, as I conceive, have fully answered the expectations of those literary persons who had patronised the undertaking; but whether the number of general readers in Ireland was insufficient in the middle of the eighteenth century to support such a periodical work, or whether it was owing to the continued ill health of the editor, I know not; but it appears that the publication was entirely discontinued in the month of June 1749, which was only one month after the

commencement of our great national work, the "Monthly Review."

It is observable that the "History of the works of the Learned" (the octavo series) was brought down to the end of the year 1743; and that the Irish Journal was begun in 1744, and carried on until the Monthly Review commenced in the year 1749. Thus the chasm which there would otherwise have been in the series of British Journals, is filled up, and we have now almost a complete succession of works of this nature in the English language from the year 1681 until the present day.

In consequence of the entertainment and instruction which I have myself derived from the perusal of the Dublin Journal, I am desirous of giving some farther particulars respecting the plan of the work, and the manner in which it was executed.

As to the plan of the publication, that will perhaps best appear from the words of the Author in the preface to the first volume. "As Foreign books," says he, "are only known from the French Journals, understood by few, and read by fewer, my intention is to give *English* abstracts of the most important foreign books, German, Dutch, French, or Latin. To execute this scheme, I shall chuse the best Abstracts to be found in the great variety of foreign Journals; enlarge upon what shall be judged to be of the greatest moment; and suppress what shall appear to be of small use. I shall also venture some short remarks of my own, when necessary to the better understanding of the subject; and sometimes give abstracts not to be met with in any Journal."

"Tho' my principal design is to give information of Foreign books, yet I do not mean so to confine myself as never to take notice of ENGLISH writers, who treat of matters either entirely new, or remarkably curious; but satyr and personal reflections, shall be totally excluded from these papers."

"The favourable reception of this undertaking must necessarily depend on the execution. Success will encourage me to go on, and to give four parts, Octavo, every year, one each quarter, containing about fourteen sheets, at the rate of one shilling and sixpence English money, each part."

" All books of note published abroad, of which no abstract is given, shall be exactly mentioned at the end of each volume, with whatever happens remarkable in the Universities of *Muscovy*, *Sweden*, *Denmark*, *Germany*, *Holland*, *Switzerland*, *and France*."

" A Table of Contents, and an Index of Authors, shall be printed with each volume. My correspondents are desired to frank their Letters, and direct them to the Reverend Mr. Droz in College Green, Dublin*."

Having thus stated the design of the author, justice to his memory obliges me to say that he has produced a very interesting work, abounding with curious and useful matter; and that the various observations and reflections which are interspersed throughout the five volumes, bespeak a man of education and discernment. Each Number contains a Review of from 10 to 14 distinct articles, which are followed by a chapter of Literary News from all the principal Cities and Towns of Europe. This Chapter of Literary News is very comprehensive, as it contains not only a Quarterly account of valuable books published in different parts of Europe; but anecdotes of learned men; an account of the formation of New printing establishments; the erection of New Academies; the founding of Literary Societies; the establishment of public Libraries; an account of New works in the press; the present employment and future engagements of men of letters; an analysis of the Memoirs of Learned Societies; and an announcement of the Death of eminent men, with an account of their principal writings, *&c. &c.* The chapter concludes with an account of the new books published during the last Quarter, in Great Britain and Ireland. A Table of contents, and a General Index closes each volume.

XXXI. 1746. " The Museum, or the Literary and Historical Register." London: Printed for R. Dodsley, in Pall-

* During the publication of this Literary Journal, Mr. J. P. Droz was an eminent Bookseller on College Green, Dublin; but whether he was related to the Rev. Mr. Droz of College Green, the learned editor of this work, I have not been able to ascertain.

mall, 1746." The name of Dodsley, at first sight, prejudiced me very much in favour of this Journal. The intention of the ingenious and accomplished Editor of this work appears to have been to unite the character of a Periodical Review of Books with that of a Literary Magazine; and in my judgment he has completely succeeded in making his volumes very entertaining and at the same time highly interesting and useful.

The work was regularly published in Numbers, one number, consisting of 40 pages, every Saturday fortnight, and thirteen such numbers formed a volume in Octavo, three of which completed the work. Although Mr. Dodsley projected this periodical publication, only one fourth share of it belonged to him, the remainder being the property of Messrs. Longman, Rivington and others.

The work consists of Literary and Moral Essays; some original poetry; Literary Memoirs, including a Review of Books; and historical memoirs, including a detailed account of the Rebellion which had just been suppressed in these kingdoms. The real nature of the work will, however, be better understood by the perusal of a few abstracts from the preface which was given with the last number of the first volume.

"The favourable reception this work has met with from those whom it is an honour to have pleased, not only calls for acknowledgment, but demands our utmost endeavours to deserve the continuance of their Patronage and Assistance. In our account of Books, we hope no author has been misrepresented, no reader justly disgusted, or greatly disappointed; and we shall endeavour to let no considerable work escape our notice."

"We take this opportunity of returning thanks to those who have favoured us with their assistance in this undertaking; and we hope, by degrees, the Learned and Ingenious, in all Arts and Sciences, will honour us with their correspondence; and by that means the Museum may become a general vehicle by which the Literati of the whole kingdom may converse with each other, and communicate their knowledge to the world. How agreeable such an intercourse must be, and how highly condu-

cive to the advancement of Literature, we need not say. We therefore flatter ourselves, that such as have any useful knowledge to communicate—or any hint that may contribute to improve the mind, polish the manners, refine the taste, or mend the heart, will be as glad of such an opportunity of communicating it, as we shall be always proud of conveying it to the Public."

So much for the language, opinions and promises of the Editor. A modern writer (Mr. Chalmers) speaking of this Journal says, " It extended to three volumes, and contains a greater " variety of original Essays of real merit, than any similar un- " dertaking—nor will this be doubted, when it is added that " among the contributors were Spence,—Horace Walpole,—the " two Wartons—Akenside—Lowth—Smart—Gilbert Cooper,— " Whitehead—Merrick—and Campbell."

Each volume contains some considerable Indexes divided into classes, thus. Index to the Essays. Index to the Poetry. Index to the account of Books. Index to the History of the present state of Europe. Index of Names to the whole. The 1st Volume has also an Index to the history of the Rebellion.

XXXII. 1749. " The Monthly Review, A Periodical Work, giving an account, with proper abstracts of, and Extracts from, the New Books, Pamphlets, *&c.*, as they come out. By several Hands. London, Printed for R. Griffiths, at the Dunciad in St. Paul's Church Yard." The first Number of this work, for May 1749, was published on the 1st day of the following month, without any Preface or Advertisement whatever. And though the editor was entirely without patronage or support, he persevered in printing the work, month after month, and continued to publish it in the most regular manner possible, for more than half a century.

A few of the early numbers were sold at sixpence, and the others at one shilling each. It having, however, been found inconvenient to delay the publication of the last number of each volume, for the sake of having sufficient time to prepare the General Index, Mr., afterwards Doctor, Griffiths announced, in the Number for

June 1753, that on the 15th July would be published, price six pence, an Appendix to the eighth volume, and that this should contain, in three sheets, the General Title, Index's, *&c.* I observe, however, that it was not until the publication of the Appendix to the 28th volume, printed in 1763, that the editor informed the public that in future he should reserve the Appendix for the review of foreign books exclusively. This plan must surely have given general satisfaction, as it has been adhered to ever since. From this time, the Monthly Review was published with the utmost punctuality; seven numbers, together with a Table of Contents, and an Index to remarkable passages, forming a volume. In consequence, however, of the increased number of books issuing from the British and foreign presses, the Proprietor at length determined that it would be advisable to alter the plan of publication; and for the future to give the work to the public in numbers of a larger size, and at an advanced price; four numbers with an Appendix, forming a volume, and three volumes instead of two, being thus annually completed. This alteration commenced with the beginning of the year 1790, and the whole of the work since published, is known by the name of the "New Series of the Monthly Review." The former Numbers of the Review, now known by the name of the "Old Series," were published with the design of forming two volumes in each year; these come down to the end of the year 1789, and make in the whole 81 volumes.

In the year 1786 Mr. Griffiths published in two volumes a General Index to the first seventy volumes of the Monthly Review, compiled by the Rev. S. Ayscough, of the British Museum. The 1st volume of this Index contains a "Catalogue of the Books and Pamphlets characterized; with the size and price of each article; to which is added an Index of the Names mentioned in the Catalogue." The second volume contains "An Alphabetical Index to all the memorable Passages, with Literary Anecdotes, Critical Remarks, *&c. &c.* contained in the Monthly Review during that period." Ten years after this Index was printed, Mr. Griffiths published a third Volume of Index to the Monthly Review, commencing with the seventy-

first and ending with the eighty-first volume. This third volume is compiled on the same plan as the former, and by the same Gentleman. The eighty-one volumes from the year 1749 to the end of the year 1789, together with the three volumes of Indexes, form a complete and highly useful set of books which are now known by the name of the first series of the Monthly Review.

The New Series commenced in the Month of January 1790. The first four months of the year, and an Appendix, formed the first volume; the title of which is as follows. "The Monthly Review; or Literary Journal, enlarged:——From January to April inclusive, 1790. With an Appendix."

> "——— Major rerum mihi nascitur ordo,
> Majus opus moveo." *Virg. Æn.* VII. 44.

> "But you who seek to give and merit fame,
> And justly bear a Critic's noble name—
> Fear not the anger of the wise to raise;
> Those best can bear reproof, who merit praise."

"London: Printed for R. Griffiths; and sold by T. Becket' in Pall-Mall. 1790."

The New series of the Monthly Review, altho' it has been indebted to a long line of writers of various talents and acquirements for the character which it has supported and for the rank which it now holds among the literary productions of the present day, seems at no time to have forfeited the good opinion of the public; for it has been regularly published every month, without intermission; and has very ably supported its credit amidst numerous competitors. In the month of May last, the 97th volume of this series was completed. Both the Series therefore now form 178 volumes, besides the General Indexes.

It may here be remarked that while Mr. Griffiths was engaged in forming the plan of the Monthly Review, the learned and accomplished Dr. Matthew Maty *, who was afterwards ap-

* Dr. Matthew Maty was the Father of the Rev. Paul Henry Maty, the author of a learned Journal in 10 volumes Octavo which commenced in 1782 under the Title of the "New Review with Literary Curiosities and

pointed Secretary to the Royal Society and principal Librarian at the British Museum, was employed in bringing out a similar work in the French Language, under the title of the "Journal Britannique." This work which commenced in January 1750 was completed in 18 very small volumes, the last of which appeared in December 1755 *. A continuation was then undertaken by Mr. De Mauve, and this was carried on to the extent of six volumes more, until the end of the year 1757.

Another respectable work was also planned about this time, entitled "The Universal Librarian," the first Number of which purports to be a Review of Books for the Months of April, May and June, 1751, price 3s. : but as I never proposed to proceed farther in my inquiries respecting the Periodical Literary Journals than to the establishment of the Monthly Review, I must hasten to conclude my account of that important work.

The editor of the Monthly Review was Mr. Ralph Griffiths, who was one of the sons of a Supervisor of his Majesties Excise in the County of Salop; but who for the sake of improvement and to gratify his taste for reading, left the country, and placed himself with a Bookseller in the Metropolis. Here, by care and frugality, he saved sufficient to enable him to begin the book-selling business on his own account; and accordingly, in the year 1747, he opened a shop in Saint Paul's Church Yard and took the sign of the Dunciad †. Here this extraordinary man formed the plan of the Monthly Review, and in two years

Literary Intelligence." In reference to his Father having been the Editor of the *Journal Britannique*, he adopted for his work the following pleasing motto :

"*Sequitur patrem, non passibus æquis.*"

* The Journal Britannique was held in high estimation by Gibbon the Historian. See Gibbon's Memoirs in Quarto Vol I. page 87.

† In 1754 Mr. Griffiths removed to the Dunciad in Paternoster Row, and in 1759 to the Dunciad, near Catherine-street in the Strand; where his shop was the favourite lounge of Dr. Goldsmith and other literary characters of the day. In the year 1764 the Name of Mr. Becket appeared for the first time, in the title-page as the publisher of the Monthly Review; Mr. Griffiths having relinquished business, and quitted London, to attend solely to his Literary Journal.

after his settlement in Saint Paul's Church yard the publication of the work commenced, as already related.

It is remarkable that Mr. Griffiths not only planned this important Work, but that he gave it to the world without any recommendatory Preface, or any encomiastic explanation whatever of the scheme of the undertaking; that he continued the publication without private patronage of any kind; and actually lived to witness its growing prosperity, and to superintend and conduct the press for fifty-four years. The profits arising from the sale of this very respectable and useful Journal, enabled him to live in a state of comfort, and latterly of opulence.—His Son, George Edward Griffiths, Esq. of Turnham Green, has continued to be the Editor ever since the demise of his Father.

It has already been observed that the contributors to the Monthly Review were Gentlemen of the most respectable characters; and I have understood that the first article in the work was written by Dr. William Rose, who kept an academy of great respectability at Chiswick, and was author of a well known translation of Sallust*. This Gentleman was brother-in-law to Mr. (afterwards Dr.) Griffiths and also to the Rev. Jabez Hirons of St. Albans; these three gentlemen having married the three daughters of the Rev. Dr. Samuel Clarke of the latter place. Another constant contributor was the Rev. William Ludlam the Mathematician, author of many esteemed works on astronomy and mathematics, Rector of Cuckfield in Sussex, and Vicar of Norton by Galby, Fellow of John's College, Cambridge, *&c. &c.* The Rev. Thomas Gwatkin, who was afterwards Professor of Mathematics and Natural Phi-

* The literary rank which Dr. Rose held with his contemporaries, may be readily conceived from the following circumstances. He was the constant friend and counsellor of Andrew Miller, the eminent Bookseller in the Strand; Dr. Lysons speaks in the handsomest way of him in his History, and Arthur Murphy wrote lines upon his death, which are inscribed upon his tomb. A further account of this excellent man, may be seen in Nichols's Anecdotes of the 18th Century, Vol. III. p. 386.

† Nicholson's Literary Anecdotes, Vol. III. page 506.

losophy in the College of William and Mary in the State of Virginia, was also an occasional writer for the work; but one of the most constant contributors was the Rev. Samuel Badcock, a respectable Dissenting Minister, who is said to have been the author of those elegant Sermons which were preached in the year 1784 at the Bampton Lectures, delivered before the University of Oxford, by Dr. Joseph White, Professor of Arabic in that University. This Gentleman, as I have understood, was connected with the Proprietor of the Monthly Review for many years.

Dr. Griffiths died at Turnham Green on the 28th September 1803, in the 83d year of his age, and Mrs. Griffiths on the 24th August 1812 ‡.

I have already said that Dr. Griffiths published two General Indexes to the First Series of the Monthly Review, consisting of 81 volumes. In the year 1818 an Index to the first Eighty-one volumes of the *New* Series was also published. This latter was compiled by Mr. J. Cudlipp, in two thick volumes, and is sold at two guineas and a half in boards. The title to this Index runs thus: "A General Index to the Monthly Review, from the Commencement of the New Series in January 1790 to the end of the Eighty-first volume completed in December 1816. In Two Volumes. London, Sold by John Porter, Pall Mall, 1818."

The Public have thus a very valuable and connected Series of Literary Journals, extending to no less than 162 volumes *, besides Five volumes of General Indexes.

A work of this magnitude, embracing a period of more than seventy years, would be an unwieldy incumbrance, and comparatively of little use were it not for the volumes of General Indexes already referred to, and which may be now had to accompany it—but with this accompaniment a Library cannot contain a set of books of more value, nor of more general utility.

Having been a regular reader of the Monthly Review for more

* As the work is still in the course of publication, 16 volumes more have been completed, but these at present have no General Index.

than Forty years, I apprehend I can form a tolerably correct opinion of the general merits of the work and of the manner in which it has usually been conducted—and I do conceive that I am justified in saying that I know of no publication in the English Language, that is better calculated to give young persons a general knowledge of men and of books, provided they acquire the habit of frequently consulting it by means of the General Indexes.

Although I never had any intercourse with the late Dr. Griffiths, nor have any personal knowledge of the present Editor of the work, I feel pleasure in availing myself of the opportunity which this History of the British Literary Journals affords me of expressing my approbation of a Book which I consider in the light of an old acquaintance from whom I have derived much valuable information, and to whom I am under no small degree of obligation.

Mecklenburgh-square,
March 20*th*, 1822.

SAMUEL PARKES.

ART. VIII. *On Prussic Acid. By* ANDREW URE, M.D., F.R.S., *&c.*

HAVING been frequently consulted by physicians and apothecaries concerning the strength of the dilute prussic or hydrocyanic acid employed in medicine, I instituted a series of experiments, to determine the relation between its specific gravity and quantity of real acid. The acid which I prepared with this view, had a specific gravity=0.957.

The following table comprehends their results.

Quantity of above liquid Acid.	Specific Gravity.	Real Acid pr. Ct.
100.0	0.9570	16
66.6	0.9768	10.6
57.0	0.9815	9.1
50.0	0 9840	8.0
44.4	0.9870	7.3
40.0	0.9890	6.4
36.4	0.9900	5.8

Quantity of above liquid Acid.	Specific Gravity.	Real Acid p. Ct.
33.3	0.9914	5.3
30.8	0.9923	5.0
28.6	0.9930	4.6
25.0	0.9940	4.0
22.2	0.9945	3.6
20.0	0.9952	3.2
18.2	0.9958	3.0
16.6	0.9964	2.7
15.4	0.9967	2.5
14.3	0.9970	2.3
13.3	0.9973	2.1
12.5	0.9974	2.0
11.8	0.9975	1.77
10.5	0.9978	1.68
10.0	0.9979	1.60

From the preceding table, it is obvious that for acid of specific gravity 0.996 or 0.997, such as is usually prescribed in Medicine, the density is a criterion of greater nicety than can be conveniently used by the majority of practitioners. In fact, the liquid at 0.996 contains about double the quantity of real acid, which it does at 0.998. It is therefore desirable to have another test of the strength of this powerful and dangerous medicine, which shall be easier in use, and more delicate in its indications. Such a test is afforded by the red oxide of mercury, the common red precipitate of the shops. The prime equivalent of prussic acid, is exactly one-eighth of that of the mercurial peroxide. But as the prussiate of mercury consists of two primes of acid to one of base, or is in its dry crystalline state, a bicyanide, we have the relation of one to four in the formation of that salt, when we act on the peroxide with cold prussic acid. Hence we derive the following simple rule of analysis. To 100 grains, or any other convenient quantity of the acid, contained in a small phial, add in succession, small quantities of the peroxide of mercury in fine powder, till it ceases to be dissolved on agitation. The weight of the red precipitate taken up being divided by four, gives a quotient representing the quantity of real prus-

sic acid present. By weighing out beforehand, on a piece of paper, or a watch glass, forty or fifty grains of the peroxide, the residual weight of it shews at once the quantity expended.

The operation may be always completed in five minutes, for the red precipitate dissolves as rapidly in the dilute prussic acid, with the aid of slight agitation, as sugar dissolves in water. Should the presence of muriatic acid be suspected, then the specific gravity of the liquid being compared with the numbers in the above table, and with the weight of peroxide dissolved, will shew how far the suspicion is well founded. Thus, if 100 grains of acid, specific gravity 0.996, dissolve more than 12 grains of the red precipitate, we may be sure that the liquid has been contaminated with muriatic acid. Nitrate of silver, in common cases so valuable a re-agent for muriatic acid, is unfortunately of little use here; for it gives with prussic acid, a flocculent white precipitate, soluble in water of ammonia, and insoluble in nitric acid, which may be easily mistaken by common observers, for the chloride of that metal. But the difference in the volatility of prussiate and muriate of ammonia, may be had recourse to with advantage; the former exhaling at a very gentle heat, the latter requiring a subliming temperature of about 300° Fahrenheit. After adding ammonia in slight excess to the prussic acid, if we evaporate to dryness at a heat of 212°, we may infer from the residuary sal-ammoniac, the quantity of muriatic acid present.

The preceding table is the result of experiments which I made some time ago at Glasgow. I have lately verified its accuracy by experiments made at the Apothecaries' Hall, London, on their pure prussic acid. 100 grains of the bi-cyanide of mercury, require for their conversion into bichloride (corrosive sublimate), 28.56 grains of chlorine, a quantity to be found in 100 grains of muriatic acid, specific gravity 1.1452. And as 100 grains of the bi-cyanide contain 20.6 of real prussic acid, they will furnish, by careful distillation on a water-bath, a quantity of liquid acid, equivalent to 700 grains of the medicinal strength 0.996. By consulting my table of muriatic acid, published in this Journal for January last, the quantity of it at any

density, necessary for decomposing the above cyanide, will be immediately found; bearing in mind, that 32.5 = the prime equivalent of salt, corresponds to 9 of chlorine.

In Dr. Thomson's System of Chemistry, it is said, that the red precipitate of mercury, is " somewhat soluble in water *." This statement, were it true, would militate against the use of that substance, as a test of prussic acid. But on digesting water with heat, on the mercurial peroxide, and passing a current of sulphuretted hydrogen through the filtered liquid, I could find no traces of mercury.

ART. IX. *On the Changing of Vegetable Colours as an Alkaline Property, and on some Bodies possessing it.* By M. Faraday, Chem. Assist., Royal Institution.

THE changes produced by acids and alkalies on vegetable colours, have long been considered as very distinctive and peculiar effects, and even sufficient of themselves to indicate the presence of these bodies. Since the introduction into those classes of substances before excluded, as of silica, various oxides, and vegetable substances, into the class of acids; and of oxide of lead, morphia, *&c.*, into the class of alkalies; it becomes more important to substantiate any particular property as peculiar to those classes, or show its fallacy, by pointing out to what substances excluded from them it also belongs.

At present I shall detail the results of a few experiments made on the colouring matter of turmeric and rhubarb, comparing the changes produced on them by alkalies to those occasioned by some other bodies. At Vol. V. p. 125, I mentioned the property possessed by muriatic acid gas and strong acids in general, of reddening or browning turmeric paper. I find that in general they have the same effect on rhubarb paper, and a very weak nitric acid gives a brown tint to it, exactly like that of an alkali: strong solution of muriatic acid does not affect it much, but sulphuric acid does.

* 6th Edition, Vol. I. page 482.

At Vol. VI. p. 152, and XI. p. 403, I have shewn the effect of boracic acid, in reddening turmeric paper. Mr. South, I believe, first shewed, that the sub-acetate of lead reddened turmeric, and this has been considered sufficient evidence by many, that the oxide of lead merited, in some degree, the name of an alkali. I find on trial, however, so many substances possessing this property, that it must either be limited more exactly than has yet been done, or else given up as a distinguishing property.

All the soluble salts of iron that I have tried, except the acetate, brown turmeric paper. Weak solutions of the green and red muriate seem very alkaline indeed, and even common green vitriol strongly so. They do not, however, produce the same effect on rhubarb paper, but the per salts give it an olive-green tint, whilst the proto salts produce no change at first, but gradually give green tints, apparently from becoming per salts.

If a strong solution of muriate of zinc be boiled on zinc, it gradually oxidizes the metal and dissolves it; and a concentrated solution is obtained which, when diluted, deposits either an oxide or a sub-muriate of zinc. This strong solution is apparently alkaline to tumeric paper. If it be diluted with about its bulk of water, and filtered, it will still redden tumeric paper, though it proves slightly acid by litmus paper. If farther diluted, more precipitate will fall, and the solution will appear alkaline or not, according as the dilution has been small or great. This substance has the same effect on rhubarb paper, and the tint is very like that of a true alkali.

The acid nitrate of bismuth appears alkaline to tumeric; if diluted till a little oxide becomes deposited, it is more so:—the common solution of chloride of antimony in muriatic acid added to water till a precipitate falls, appears alkaline:—per muriate of tin produces a strong change; proto-muriate of tin a very decided redding; sulphate of tin, slight only. When the acid nitrate of bismuth, and the three salts of tin mentioned are applied to rhubarb paper, they produce but little effect at first, but if dried by the fire become quite brown.

A strong solution of chloride of manganese seems feebly but evidently alkaline to tumeric paper.

It may be supposed, that with many of these substances, the effect is produced principally by the acid present, inasmuch as they were all more or less acid to litmus, and it has been shewn that these bodies, especially nitric acid, have this power. But the very slight excess in some of them, as in the salts of iron, zinc, manganese, *&c.*; and the greater effect produced by farther dilution, as with bismuth and antimony, shew that the whole substance must act: for if it be considered due to the acid alone, it is not, in the first case, of sufficient quantity, and in the second, it would be diminished by dilution.

It is probable that, if the tints produced be compared exactly with those occasioned by alkalies, slight differences might be perceived among some of them; or that, if the properties of the altered colouring matter were examined, they would be found different with the different substances, as M. Desfosses * has shewn with regard to boracic acid and other acids; but my object has not been to trace these changes as far as possible, but merely to shew their general appearance; to guard against any deceptive conclusion with respect to solutions tested by tumeric; and to call attention to the distinguishing characters of acids and alkalies.

Art. X. *Proceedings of the Royal Society.*

The following papers have been read at the table of the Royal Society since our last report.

March 28. Contributions towards the anatomy of whales, by Mr. William Scoresby, communicated by Sir Everard Home, bart., V. P. R. S.

April 18. On the changes that have taken place in the declination of some of the principal fixed stars, by John Pond, esq., Astronomer Royal, F. R. S.

Extract of a letter from Captain Sabine to the President.

Some observations on the buffy-coat of the blood, by John Davy, M. D., F. R. S.

* Vol. xi. 403.

April 25. Some observations made preparatory to the eclipse of 21st August, 1821, and of the eclipse itself, by Mr. William Dawes.

On the mechanism of the spine, by Henry Earle, esq., F.R.S.

May 2. On the nerves which associate the muscles of the chest in the actions of breathing, speaking, and expression, by Charles Bell, esq.

A short account of some appearances in the moon, 24th April, 1822, by Henry Lawson, esq.

9. Experiments and observations on the pitch stone of Newry, and its products, and on the artificial formation of pumice, by the Right Hon. George Knox., F. R. S.

16. Observations on the changes the egg undergoes during incubation in the common fowl, by Sir Everard Home, bart., V. P. R. S.

23. On the mathematical laws of electro-magnetism, by Peter Barlow, esq.

Observations on the heights of places in the trigonometrical survey, by B. Bevan, esq.

June 6. On the binomial theorem, by John Walsh, esq.

Some observations on corrosive sublimate, by John Davy, M.D., F.R.S.

13. On the state of water and aëriform matter in the cavities of certain crystals, by Sir Humphrey Davy, Bart. P. R. S.

20. Some experiments on the changes which take place in the fixed principles of the egg, by Wm. Prout, M. D., F. R. S.

27. On the ultimate analysis of animal and vegetable substances, by Andrew Ure, M. D., F. R. S.

Art. XI.—ANALYSIS OF SCIENTIFIC BOOKS.

i. *De l'emploi du Chalumeau, dans les Analyses Chimiques, et les Déterminations Minéralogiques. Par* M. Berzelius. *Traduit du Suedois, par* F. Fresnel. *A Paris,* 1821.

The Use of the Blow-pipe in Chemical Analysis, and in the Examination of Minerals. By J. J. Berzelius. *Translated from the French of* M. Fresnel, by J. G. Children, F. R. S., &c, *London,* 1822.

In the first volume of this Journal, we noticed a former work by M. Berzelius, entitled " An Attempt to establish a Pure Scientific System of Mineralogy," &c., and gave our opinion very decidedly, of the complete failure of that " attempt." It is highly gratifying, that we are enabled to make a very different report of the work now before us, and to speak almost as much in its praise, as we did in censure of its predecessor. Before we proceed to the " blow-pipe," however, we must have a few more words, and they shall be the last, with the " attempt."

Our review was of the English translation of that work, by John Black, London, 1814. A copy of the original, Dr. Thomson tells us, in the advertisement, was sent him by Professor Berzelius; on looking over which, " it appeared highly deserving the attention of mineralogists, and likely, if properly followed up, to occasion a most important improvement in the method of analyzing minerals, and in the scientific arrangement of them." He therefore engaged his friend, Mr. Black, to translate the work, who stipulated that the Doctor should compare his manuscript with the original, and " take care that it every where conveyed its sense." " This task," says the Doctor, " *I performed with all requisite attention; and can, I think, answer with some confidence for the fidelity of the translation.*" How carefully he revised his friend's manuscript, and how well founded his confidence in its fidelity to the original was, we cannot decide directly from our own knowledge, as we have not the Swedish copy before us. Berzelius, however, has complained bitterly of its infidelity, in the Introduction to a fresh translation which he caused to be published in French, under his own immediate inspection, in 1816 *, in consequence of the errors contained in the English copy. Let him speak for himself. " La traduction Anglaise du même ouvrage, fut faite malheureusement par quelqu'un *qui n'a sans doute aucune notion de minéralogie, car elle est pleine d'erreurs;* et dans beaucoup

* We had not heard of that translation, when our first volume was published.

d'endroits, *le sens de l'original est tout-à-fait défiguré.* Elle parut cependant précédée d'une préface du Docteur Thomson, *qui assurait l'avoir confrontée à l'original, et qu'elle était parfaitement fidèle.* Sur ce temoignage, le redacteur du Journal de Physique en fit faire une version en Français qui renferme naturellement toutes les fautes de la traduction Anglaise. C'est donc par le desir de voir cet ouvrage jugé tel qu'il est, que j'ai publié une traduction d'après l'original Suédois, et sans autres changements que la correction de quelques nombres, que j'ai taché de rendre plus exacts; la substitution du résultat de nouvelles analyses à d'autres moins parfaites, et le remplacement de quelques exemples par d'autres mieux choisis." Under these circumstances, we have felt it our duty to compare the English translation of 1814, with the French of 1816, for which we have the author's assurance in the words just quoted, that it is *really* faithful to the original Swedish, an assertion in which Dr. Thomson necessarily acquiesces, as he has not any where, that we know of, called it in question. We had scarcely begun this hard task, before it was evident that the Black and Co. translation is any thing but literal, and in some instances the sense, even of the passages, is perverted. Dr. Thomson did well to speak with caution of its fidelity. We forbear to quote the parallel passages alluded to, because we do not think it worth while to employ our reader's time or our own so unprofitably; but, if any one doubt our accuracy, he may soon cure his scepticism by referring to the two books. He need not look far for ample proof of the truth of our assertion. The excuse for all this we suppose will be, that they translated from the original Swedish, and we have been comparing their translation with the French. The apology, however, is inadmissible, for Berzelius has declared the French translation to be faithful to its prototype, and we cannot but believe that he is quite as conversant with the Swedish and French languages as Messrs. Black and Thomson are with the Swedish and English.

But the point that chiefly concerns us is, has the English translation perverted the general views of the original work, and especially those passages, which have been commented on by us? For it is, in fact, the original which has been on its deliverance at the bar of critical justice; though the translation comes in of course as accessary after the fact, and must also plead, (guilty, we fear,) to a separate indictment for its own peculiar sins of inaccuracy and error.

The note respecting the quantity of oxygen in the earths, and the method of ascertaining it in silica, is very much shortened in the French translation, the passages quoted by us being wholly omitted. We cannot decide, therefore, who has best right to the nonsense it contains, the author or his translators. It is not necessary, however, as we have said before, to trouble

our readers with particular passages, since, after having carefully compared the two translations, we do not find any sufficient reason to induce us to revoke our opinion of this *nouveau systême*.

We have been guilty ourselves of an omission, in the review alluded to, in having left unnoticed the *signs* which Berzelius has invented, to express the composition of chemical compounds and minerals; we shall therefore devote a few words to them in this place. " When we attempt," says our author, " to express chemical proportions, we perceive the necessity of having chemical signs." (*Essai sur la Theorie des Proportions Chimiques*, p. 109,) and accordingly he adopts two kinds, one chemical, the other mineralogical. With the former, 1st. a simple substance not metallic (which he calls a metalloid), is denoted by the initial letter of its Latin name, although it may be also common to the name of some metal; 2d. a metal having the same initial as another metal or a metalloid, is denoted by the two first letters of its name, or should these be common to some other name, then the first different consonant is added to the initial letter instead. Thus O. = oxigenium, S. = sulphur, Si. = silicium, St. = stibium, Sn. = stannum, &c. A chemical sign only indicates a single atom; to denote a greater number, a figure is placed to the left of it, like an algebraical coefficient: thus Cu + O, signifies protoxide of copper, and Cu + 2 O peroxide of copper. But since oxygen enters into most combinations, and often in the proportion of several atoms, instead of employing its proper sign O, with or without its coefficient, the number of its atoms is indicated by points placed over the base, each point denoting an atom of oxygen. Thus $\dot{\mathrm{Cu}}$ = protoxide of copper, $\ddot{\mathrm{Cu}}$ = peroxide of copper, $\ddot{\mathrm{Su}}$ = sulphurous acid, and $\dddot{\mathrm{Su}}$ = sulphuric acid, &c. Proto-sulphate of copper is expressed by combining these two signs, as $\dot{\mathrm{Cu}}\,\dddot{\mathrm{S}}$, and the persulphate by $\ddot{\mathrm{Cu}}\,\dddot{\mathrm{S}}^2$ the exponential figure, [2] denoting that it contains two atoms of acid. The composition of a salt with more than one base is expressed by connecting the signs by the usual algebraic mark of addition. Sulphate of potassa and alumina, for instance, is denoted by $\ddot{\mathrm{K}}\,\dddot{\mathrm{S}}^3 + 2\,\dddot{\mathrm{Al}}\,\dddot{\mathrm{S}}^3$; (K being assumed to represent *kalium*, i. e., potassium,) and crystallized alum, in which these ingredients are combined with several atoms of water (expressed by $\mathrm{H}^2\,\mathrm{O}$.) is represented by $(\ddot{\mathrm{K}}\,\dddot{\mathrm{S}}^3 + 2\,\dddot{\mathrm{Al}}\,\dddot{\mathrm{S}}^3) + (48\,\mathrm{H}^2\,\mathrm{O}.)$ (See Essai, p. 113) The mineralogical signs are formed on a similar model, but are more simple. They consist of the initials of the elementary bodies in the italic character, the dots denoting the atoms of oxygen are omitted, and water is represented merely by Aq. Black Siberian Mica for instance is denoted

by $K S^3 + 2 M S + 3 A S + 4 F S$ meaning that it is composed of one atom of trisilicate of potassa, two atoms of silicate of manganese, three atoms of silicate of alumina, and four atoms of silicate of iron! Now we beg to dissent *in toto* from the assertion that these or any other signs are at all necessary, and we contend that they are calculated rather to mislead and mystify, than facilitate our progress, or elucidate our results. If we gain any thing in brevity, (and very little do we even in that respect) it is at the expense of great risk of confusion, both from that misinterpretation which all symbols are liable to, as well as from the inaccuracies they are likely to introduce, by errors in transcribing, or of the press. Even in point of brevity, the advantage is merely in the act of committing them to paper, for when they are to be deciphered, we must, mentally at least, read them at full length, if we would perfectly comprehend their import. It is not with these symbols as with the figures in arithmetic, for they are connected with the various operations of addition, subtraction, and multiplication; which could hardly be performed, were the quantities expressed by words. A peculiar method of abbreviated notation therefore in that case is absolutely necessary, but the chemical and mineralogical symbols, are mere expressions of simple aggregates, and when once noted down are capable of no further operation. Hence common language is amply sufficient for every purpose these symbols can be applied to. There is, besides, a certain air of mathematical parade in these symbols which is not pleasing; the connecting sign +, with their coefficients and exponents, gives them so strong a resemblance to algebraical formulæ, that on opening at some of the pages of the *Nouveau Systeme,* we might almost fancy we had taken up a volume of Bonnycastle's Algebra.

Before we quit the subject of mystification, we shall say a few words on a propensity which has sprung up since the establishment of the atomic theory, to entangle chemical calculations in unnecessary algebraical formulæ. In the sixth volume of the *Annals of Philosophy,* p. 321, there is a highly interesting and valuable paper on the relation between the specific gravities of bodies in their gaseous state, and the weights of their atoms. The name of the author is not annexed, but it is now well known to be by Dr. Prout. We select this paper as a case in point, (though similar instances are but too numerous) because its excellence demands that it should be universally known and understood, which, as the calculations whence the results are derived are algebraical, it can hardly be by all who cultivate the science of chemistry. These calculations are not, indeed, absolutely necessary to render the general object of the essay intelligible, and are therefore subjoined in the form of notes; but, however confidently the reader may rely on the accuracy of an author's

results, it is always satisfactory as well as instructive to trace the steps by which he arrives at them; and hence it is desirable that they should be detailed in the plainest possible form, and when the subject will admit of it, and it does not involve tedious and periphrastic operations, by the method of common arithmetic. Now every one of the calculations in the notes to this paper, might just as well have been worked by the common rule of three, as by algebra. For instance, let us take the first calculation. Atmospheric air is constituted by bulk of four volumes of azote, and one volume of oxygen, " and if we consider the atom of oxygen as 10, and the atom of azote as 17.5, it will be found to consist by weight of one atom of oxygen and two atoms of azote, or per cent.

Oxygen .... 22.22.
Azote .... 77.77.

From these data the specific gravities of oxygen and azote, (atmospheric air being 1.000) will be found to be,

Oxygen 1.1111
Azote 0.9722" (An. Ph. vi. 322.)

To which the following formula is subjoined.

Let $x =$ sp. gr. of oxygen. $22.22 = a$.
$y =$ sp. gr. of azote. $77.77 = b$.

Then $\frac{x + 4\,y}{5} = 1$

And $x : 4y :: a : b$.

Hence $5 - 4\,y = \frac{4\,a\,y}{b}$

And $y = \frac{5\,b}{4\,a + 4\,b} = 0.9722$, and $x = 5 - 4\ y = 1.1111$.

Now since there are five equal volumes of gases in the mixture, which together compose the atmospheric air whose specific gravity is unity, the specific gravity of each of the component gases must be in the ratio of the weight of one volume of either of them respectively, to that of $\frac{1}{5}$th of the volume of the compound produced; for there is no change of volume on their being mixed; that is, calling the volume of air produced 100.

$$20 : 22.22,\ \&c. :: 1.00 : 1.1111$$
$$\text{and } 20 : 19.44,\ \&c.\ \left(\frac{77.77}{4}\right) :: 1.00 : 0.9722.$$

And so for the other calculations of the specific gravity of hydrogen, chlorine, *&c.*

It will hardly be supposed that we mean to question the importance of algebra in a general sense; no one can be more conscious than ourselves, how indispensably necessary it is to have not merely a slight, but a full and perfect knowledge of that science in a great variety of complicated and abstruse investigations; it is only in cases like the present, where no *dignus vindice nodus* exists, that we protest against its introduction, for even such a simple formula as the preceding tends to perplex a numerous class of readers, and, as to them at least, to obscure rather than illustrate. Plain facts should be stated in plain terms; to introduce extraordinary means for accomplishing ordinary ends, is like Hogarth's philosopher using complicated machinery to draw a cork. We would say therefore to the mathematical chemist, enjoy the advantage which your algebra gives you in your closet, avail yourself of all its powers to abbreviate your calculations and elucidate your theoretical speculations, but when you detail them to the general mind, let them be clothed in the plainest garb your genius can invest them with; and in all cases, unless it be absolutely necessary, instead of saying let x be this or that, we say let x alone.

We perfectly agree with Dr. Thomson in the well-merited eulogy he has pronounced on Dr. Prout for the essay alluded to, (*Annals of Philosophy*, xvi. 167); but when he asserts that "mere experimenters may relinquish the field, for there is not a great deal more which they can do," we consider him as endeavouring with the whole weight of his authority, whatever it may be worth, fatally and effectually to oppose the progress of chemical science. If by the mere experimenter he mean a foolish fellow, who swelters at the furnace, and half suffocates himself with the fumes from his sand bath without any determinate object in his labours, or with views as chimerical as the philosopher of Laputa who tried to make deal boards out of saw-dust, well and good! but if he mean, as he obviously does, that a knowledge of algebra and the mathematics is *essentially necessary* to the *chemist*, we deny the assertion. An ingenious and skilful operator thoroughly conversant in chemical philosophy, but with merely a competent knowledge of common arithmetic, is as likely to devise and execute important experiments as the best algebraist that ever combined the knowledge of the two sciences. The whole of Dr. Black's theory of latent heat is developed in his lectures edited by Dr. Robison, without the introduction of a single algebraical formula in the text; whence it is clear that Black did not think algebra necessary to explain his views, even on so difficult a subject. Would Dr. Thomson, who sets himself up as the autocrat of chemistry, have presumed to have ordered *him* to relinquish the field? But it is time to turn to the volume that lies open before us, and we do so with pleasure.

"The subject of the work now offered to the public," says

the author, very truly, in his introduction, "is highly interesting to the practical chemist, the miner and the mineralogist. It is a system of chemical experiments, made in the dry way, as it is called, and almost always on a miscroscopic scale, but which presents us in an instant with a decisive result." It is not less true, that the way in which he has treated it, is as satisfactory as the subject is important. Mr. Children (whose translation we shall more particularly notice hereafter, adopting it generally in the mean time, in the passages we may have occasion to quote) observes in his preface, "The name of Berzelius, as a skilful and patient experimenter, stands almost unrivalled; and the present essay amply vindicates his claim to the high reputation he has acquired. It is an invaluable collection of important and new facts, and admirably supplies the want which has long been felt and acknowledged of a scientific practical treatise on the blow-pipe." (Trans. preface, v.) The observation is just in every respect.

The subject is treated in the following order. History of the blow-pipe.—Description of the blow-pipe.—The combustible—the blast and flame—the support—additional instruments—the re-agents and their use—the habits of the pure alkalies, earths, metallic oxides, sulphurets, alloys and acids, before the blow-pipe, and the pyrognostic characters of minerals and urinary calculi.

Lamps are recommended to be used with the blow-pipe, in preference to candles, as "the radiant heat from the substance under examination melts the tallow or wax, and occasions them to burn away too fast; and besides common candles do not always furnish sufficient heat." "The best fuel for the lamp is olive oil."

Under the head "*blast* and *flame*," are given very ample and accurate directions how to keep up a steady blast, and produce a strong heat, and the following instructions for oxidation and reduction are very important.

"*Oxidation* ensues when we heat the subject under trial before the extreme point of the flame, where all the combustible particles are soon saturated with oxygen; the farther we recede from the flame, the better the oxidation is effected (provided we can keep up sufficient heat;) too great a heat often produces a contrary effect, especially when the assay is supported by charcoal. Oxidation goes on most actively at an incipient red heat. The opening in the beak of the blow-pipe must be larger for this kind of operation than in other cases.

"For *reduction*, a fine beak must be employed, and it must not be inserted too far into the flame of the lamp; by this means we obtain a more brilliant flame, the result of an imperfect combustion, whose particles, as yet unconsumed, carry off the oxygen from the subject of experiment, which may be considered as

being heated in a species of inflammable gas. If in this operation the assay become covered with soot, it is a proof that the flame is too smoky, which considerably diminishes the effect of the blast. Formerly, the blue flame was considered as the proper one for the reduction of oxides, but this idea is erroneous; it is in reality the brilliant part of the flame which produces deoxidation; it must be directed on the assay so as to surround it equally on all sides, and defend it from the contact of the air."

"A very advantageous mode of practice, in order to acquire the art of making a good reducing flame, is to fuse a small grain of tin, and raise it to a reddish white heat on a piece of charcoal, so that its surface may always retain its metallic brilliancy. Tin has so great a tendency to oxidation, that the moment the flame begins to become an oxidating one, it is converted into an oxide of tin, which covers the metal with an infusible crust. We must begin by operating on a very small grain, and gradually proceed to larger and larger. The greater the quantity of tin that he can thus keep in a metallic state, at a high temperature, the more expert is the operator in his art."

The supports are charcoal, platinum foil, platinum wire, plates of mica, glass tubes, open at both ends, and glass matrasses.

The best of these, except in particular cases, is the platinum wire; and we can confirm, by our own experience, its superior convenience. "The diameter of the platina wire is arbitrary; the finest is the best, provided it be thick enough not to bend with the blast; if too thick, it absorbs too much heat." We find a wire of 1—100th of an inch in diameter, answer the purpose excellently. The mode of using it is thus described: It is to be "bent at one end into a hook, which serves as the support in the following manner. Having moistened the hook with the tongue, it is to be dipped into the flux, a portion of which will adhere to it; this is to be fused by the lamp into a globule, which congeals and adheres to the curvature; the assay must then be moistened, to make it adhere to the flux, which is now solid, and the whole heated together. We thus obtain an insulated mass, which may be conveniently examined without danger of our being deceived by the appearances which sometimes ensue on charcoal from the play of colour, when the assay globule is detached on a black ground. This method is much superior to either of the preceding, and so completely answers the purpose, that, in most cases, the platina wire is preferable to charcoal, especially when the reduction of a metallic oxide is to be avoided. Generally speaking, all oxidations should be performed with the platina wire, as well as those reductions in which change of colour is the only object in view."

"*Plates of mica* may be used in roasting ores, when the de-

oxidating effect of charcoal on the parts of the assay in contact with it, might be injurious.

"Glass Tubes.—When it is necessary to roast a substance to ascertain what it is combined with, I use a glass tube, two inches long at least, and about one-eighth of an inch in diameter, and open at both ends; I introduce the assay into it at a little distance from one of the ends, and, according to the temperature required, I heat the tube either with the spirit lamp or with the common lamp by means of the blow-pipe, inclining it more or less (the part containing the assay being held downwards) as it is necessary to pass a more or less rapid current of air over the assay. The volatile substances, not permanently gaseous, sublime, and condense, in the upper part of the tube, where their nature may be ascertained." When charcoal is used as the support, the author directs the assay to be laid with its flux on "one of the ends perpendicular to the layers of the wood; if placed on the section parallel to the layers, it would spread over the surface. Although the space between the woody layers may consume faster than the layers themselves, there is this advantage attending it, that the assay rests in that case merely on their summits, and thus often has only two or three points of contact with the charcoal."

The re-agents are carbonate of soda, borax, salt of phosphorus, (double phosphate of soda and ammonia), saltpetre, vitrified boracic acid, gypsum and fluor spar, nitrate of cobalt, tin, iron, lead, bone-ashes, silica, and oxide of copper. The three first are the principal, the others are only wanted occasionally. It is absolutely necessary that the re-agents should be quite pure, for obtaining them in which state, and the method of applying them, ample directions are given. Those respecting the use of soda are very minute and valuable. This re-agent is of great service in the fusion of bodies, and the reduction of metallic oxides. Gahn's method of discovering very minute portions of metal, reduced on charcoal by means of soda, deserves particular attention.

Borax is employed to effect the solution or fusion of a great number of substances. Its use "is founded on the tendency of its component parts to form compounds that are all fusible, though in different degrees. On one hand, it dissolves bases, and forms with them a fusible double salt with excess of base; on the other, it dissolves acids, amongst which I place silica, and even to a certain extent alumina, and forms with them acid and fusible double salts. As all these salts commonly preserve their transparency on cooling, we can hence judge the more certainly of the colour which the compound acquires from the substance dissolved."

Salt of phosphorus "acts as a re-agent, principally by

means of its free phosphoric acid; and the advantage of employing this salt, rather than the acid itself is, that the latter is very deliquescent, much more costly, and is readily absorbed by the charcoal. The salt of phosphorus, therefore, shews the action of acids on the assays; its excess of acid seizes on all their bases, and forms with them more or less fusible double salts, whose transparency and colour may then be examined. Consequently, this flux is more particularly applicable to the examination of metallic oxides, whose characteristic colours it developes much better than borax.

"The same flux exerts a repellant action on acids. Those which are volatile sublime, whilst the fixed acids remain in the mass, and divide the base with the phosphoric, or yield it entirely to it; in the latter event, they remain in suspension in the glass, without dissolving in it. In this respect the salt of phosphorous is a good re-agent for the silicates; it sets the silica free, which then appears liquefied in the salt as a gelatinous mass."

Saltpetre is useful in detecting small portions of manganese. Vitrified boracic acid, in conjunction with iron, for ascertaining the presence of phosphoric acid; gypsum and fluor spar, are used mutually to detect each other, and have no other use. Nitrate of cobalt in solution in water, is employed to ascertain the presence of alumina and magnesia; with the former it gives a blue colour, with the latter a pale rose colour. The directions given for applying this test must be carefully attended to, to avoid equivocal results. Tin, in the state of foil, is used to promote the reduction of metallic oxides; iron, to precipitate certain metals, as copper, lead, nickel, and antimony, and to separate them from sulphur, or fixed acids. Lead is employed in cupellation with bone-ashes.

"The bone ashes must be reduced to a very fine powder, a small quantity of which is to be taken on the point of a knife moistened with the tongue, and kneaded in the left hand, with a very little soda, into a thick paste. A hole is then made in a piece of charcoal, and filled with the paste, and its surface smoothed by pressure with the agate pestle. It is then to be gently heated by the blow-pipe, till it is perfectly dry, (the soda only assists the cohesion, and may be omitted). The assay, previously fused with lead, is placed in the middle of the little cupel, and the whole heated by the exterior flame. When the operation is finished, the precious metals are left on the surface of the cupel. This experiment is so delicate, that grains of silver visible to the naked eye, and indeed such as may be collected by the forceps and extended under the hammer, may in this way be extracted from the lead met with in commerce."

Silica, fused into a glass with soda, detects the presence of sulphur or sulphuric acid; and oxide of copper is used to de-

tect the presence of muriatic acid. The application of the last test is very ingenious.

"We fuse oxide of copper with salt of phosphorus into a dark-green globule, we then add the assay, and heat the whole before the blow-pipe. If it contain muriatic acid, the globule is surrounded by a fine blue flame, inclining to purple, which continues as long as any muriatic acid remains in the assay. Not one of the other mineral acids produces a similar phenomenon, and such of them as form cupreous salts, which do of themselves colour the blow-pipe flame, lose that property when combined with salt of phosphorus. For instance, the earthy mineral, in which the blue carbonate of copper (from Chessy, in France) occurs, communicates an intense green colour to the flame when heated before the blow-pipe; but when treated with salt of phosphorus, previously saturated with oxide of copper, not the slightest colour is any longer perceptible in the flame."

Under the head of *General Rules for Experiments with the Blow-pipe*, much valuable information as to the size of the assay, and the order to be observed in experimenting, is given, which our limits will not allow us to detail. We recommend a careful perusal of them to our readers.

The next division treats of the phenomena presented by different mineral substances before the blow-pipe. The experiments were made, as the author assures us in his introduction, on pure substances, and present a number of instructive and valuable facts. The effects produced by the fluxes on oxide of titanium, both pure and mixed with iron, and the means of distinguishing it from the glasses of ferruginous tungstic acid, ferruginous antimonious acid, and oxide of nickel, which assume the same shade as ferruginous oxide of titanium in the reducing flame, are very minute and important.

Oxide of cadmium, "on charcoal is dissipated in a few seconds, and the charcoal is covered with a red or orange-yellow powder. This phenomenon is so marked with oxide of cadmium, that minerals, which, like carbonate of zinc, contain one or two per cent. of carbonate of cadmium, when exposed for a single instant to the reducing flame, deposit, at a little distance from the assay, a yellow or orange-coloured ring of the oxide, most distinct when the charcoal is cold. This ring forms long before the oxide of zinc begins to be reduced, and if flocculi of that metal appear at the same time, it is a proof that the blast has been pushed too far; but if we can discover no yellow trace before the fumes of zinc begin to condense on the charcoal, we may conclude that the assay contains no cadmium."

Mr. Children has added the following in a note, on this subject, which he had from the late Dr. E. D. Clarke. "If platina foil be used for the support, instead of charcoal, the yellow or

orange-coloured ring is not only conspicuous upon the platina, but by exposing it to the point of the blue flame, the metal is revived, and afterwards deposited upon the platina, during its combustion, in the form of a protoxide, exactly like polished bronze or copper *."

The method of distinguishing bismuth from antimony and tellurium is very satisfactory, as well as the following for detecting very minute portions of tin and copper. "With soda, on the platina wire, oxide of tin combines with effervescence, forming an infusible turgid mass, insoluble in an additional quantity of soda. On charcoal it is readily reduced into a globule of metallic tin. Some native oxides of tin, especially such as contain columbium, are not easily reduced with soda, so that, on a first experiment, one might doubt its presence; but if we add a small quantity of borax, the reduction is immediately accomplished.

"Tin often occurs in nature as an accidental, and relatively, very small constituent part of the ores of columbium, titanium, and uranium, and perhaps of some others, where its presence would be little suspected in experiments made in the moist way; but when treated with soda in the reducing flame, particularly after separating the iron, metallic tin is always detected, even though it enter in no larger proportion than 1-200th part of the weight of the ore. If the proportion of iron be inconsiderable, its reduction may be prevented to a certain point, by adding borax to the soda."

Oxide of copper, "with salt of phosphorus, fuses and exhibits the same shades as with borax. If the proportion of copper be inconsiderable, the glass sometimes becomes transparent and ruby red, after exposure to the reducing flame, nearly at the moment of congelation. Commonly the glass becomes red and opaque like enamel. If the quantity of copper be so small that the character of the protoxide cannot be developed by the reducing flame, we add a little tin to the assay, (whether the flux be salt of phosphorus or borax,) and immediately continue the blast. The before colourless glass now becomes red and opaque on cooling. If the blast be kept up too long, the copper precipitates in the metallic state, particularly with salt of phosphorus, and the colour is destroyed.

"With soda on the platina wire, oxide of copper fuses into a fine green glass, which loses its colour and transparency on cooling. On charcoal the mass is absorbed and the oxide reduced. There is probably no other possible method of discovering such minute proportions of copper as may be detected by the blow-pipe, in all cases where it is not combined with other reducible metals, liable to disguise its properties. In the

* If we not mistake, the invention of this process should be ascribed to Dr. Wollaston.

latter case we must employ borax and tin. If copper and iron be found together, the same operation reduces each separately into distinct particles, which may be known by their respective colours, and separated by the magnet."

The methods of examining the sulphurets, arseniurets, and alloys, and the characteristic effects of acids, as component parts of salts, are given with equal perspicuity and detail, and the mode discovered by the author of detecting phosphoric acid, by means of boracic acid and iron, is ingenious and useful.

The last division is devoted to the pyrognostic characters of minerals and urinary calculi, and constitutes the largest portion of the work. In order to shew our readers the way in which this important part of the subject is treated, we subjoin an instance, taken at random, with which we shall conclude our extracts.

"Chondrodite, from Pargas, Åker and America. The Brucite of the Americans*. Alone, in the matrass, blackens by heat, but gives no appreciable quantity of water; the black colour disappears by roasting in the open air.

"On charcoal it is infusible. The most ferruginous chondrodite becomes opaque and brown on the points that are most strongly heated. The variety containing less iron, that from Åker, for instance, becomes milky white, by the effect of heat.

"With borax fuses slowly, but completely, into a transparent glass, slightly tinged by iron. If the glass be saturated with the chondrodite, it loses its transparency by flaming; it does not, however, become milky white, but semi-translucid, and crystalline.

"With salt of phosphorus decomposes pretty easily, and leaves a semi-transparent siliceous residuum; the glass is clear and colourless, but becomes opaline on cooling. A small quantity of soda transforms it into a difficultly fusible grey scoria; with a larger quantity it intumesces and becomes infusible.

"With solution of cobalt, in a strong heat, it gives a pale red colour, not pleasing to the eye. Chondrodite from Pargas gives a brown grey, the action of the iron preventing that of the oxide of cobalt on the magnesia."

The portion of this work devoted to the examination of urinary calculi, though brief, will be found of great utility and importance, especially to medical men, to whose attention we earnestly recommend it, as "simple and infallible, and requiring no more chemical knowledge than every physician ought to possess."

* "*Silicate of Magnesia.*—Almost all the magnesian silicates contain a combustible substance, which becomes charred when heated in close vessels. If the mineral be then heated in the open air, the charcoal burns away, and the black colour disappears. Steatite is a remarkable instance." B.

To this general analysis, and the copious extracts which we have given of the work before us, it is unnecessary to add much more; the latter prove our opinion of its merits, for we should not have thought it necessary to lay so much of its contents before our readers, were we not desirous by the specimens we have furnished to induce them to form a more intimate acquaintance with the work itself, and we have no hesitation in assuring them that they will be amply rewarded for their trouble. The part we like least is the mineralogical arrangement adopted by the author, the continual recurrence of his abominable formulæ, and his whimsical speculations on isomorphous bases, which have led him into some tedious and unprofitable details on the subject of the amphiboles, pyroxenes and garnets. Mr. Children has, very properly in our estimation, wholly omitted the formulæ, translating them into plain English in notes at the bottom of the page; we wish he had exerted the same discretionary judgment with respect to the *isomorphisms*, and left them out likewise. It remains to say a few words respecting the translation.

In the first place, with respect to its fidelity, we have been at the pains of carefully comparing it with the original, and have found it at once clear and correct. The original is closely adhered to, except in a few instances of trifling omissions, and others of judicious curtailments to which we have above adverted, which it is unnecessary to particularize, as the translator has fully explained his reasons for them in his preface. We wish that it were more common, for gentlemen of Mr. Children's rank and attainments in science, to favour the world with similar productions, for there are few means better calculated for the promotion of natural knowledge, than translations of the works of learned foreigners, accompanied with notes and illustrations by those who are equally acquainted with the subjects of which they treat, and with their authors; and who will condescend to add their stock of information to that which has been collected by others.

We scarcely regretted, though (considering the quarter whence it originated) we were somewhat surprised, at the mistake of the *rule of two*, or the having overlooked the instances where the author, *for theoretical reasons*, has doubled the atomic numbers. It is an admirable illustration of the danger which we have so often pointed out, of the system of halving and doubling *ad libitum*. The error, as Mr. Children observes, (for it was discovered in time to be corrected in a note, though not in the text,) is wholly and absolutely unimportant to the main object of the work; and we cordially agree with him, that "though it certainly is no excuse for his fault, it is one proof amongst a thousand of the danger of involving plain matter of fact in unnecessary hypothetical dogmas."

ii. Dr. Thomas Thomson—*and his* "Answer."

It is the practice of every vice, to assume the garb and name of its neighbour virtue. Thus avarice becomes frugality; chicanery, knowledge of the world; violence of temper, warmth of feeling; rudeness of manners, plain dealing; envy and detraction, the love of truth and justice. We have been presented with an amusing specimen of this masquerade, in Dr. Thomson's pretended "Answer" *, to our Review of his System, published in this Journal fifteen months ago; and were we actuated by personal hostility, we should have rejoiced at its obliquity and intemperance. We apprehend that the misstatements and abuse with which he has sullied the pages of Mr. Phillips's Journal, are the signs of a desperate cause; they are, at all events, ill calculated to erase the impression which our Review has made upon the public mind respecting the merits and accuracy of the "System."

In presenting our readers with a few additional remarks in reply to Dr. Thomson's *Answer*, we shall not degrade either ourselves, or our Journal, by imitating the coarseness of his invective, nor shall we delay until we have availed ourselves of the Regius Professor's obliging invitation to attend his lectures at Glasgow—having already had the gratification of listening to a portion of one of his discourses at the Surry Institution, near Blackfriar's Bridge, London. We shall, therefore, at once proceed to the business before us; and we pledge ourselves to defend every statement, and corroborate every assertion of our former review.

In the first place we have accused Dr. Thomson of speaking too favourably of himself, but we are sorry to observe from his "Answer," that he has not profited by our advice; and, that he is forgetful of the old proverb "*propria laus sordet*," of the truth of which he furnishes a most diverting example.

Speaking, for instance, of his *System* he says, it is a work "which has in some measure stamped the character of every systematic treatise, *both in Britain and America, and even on the continent of Europe*; and which has been sanctioned by the almost unqualified approbation of the most eminent chemists in Britain, France and Germany." Of his own individual accomplishments he says in the same tone, "As I have uniformly prided myself in the honesty, sincerity, and independence of my character; as I have been at considerable pains to give credit to whom credit was due; as I have uniformly both in my System, and in the Annals of Philosophy, while I continued its editor, given the merit of every chemical fact to

* Annals of Phil. April 1822.

the original discoverer of it, as far as my knowledge of the subject enabled me to go; as I am not conscious of any wilful misrepresentation, or twisting of facts, to serve any particular purpose; I should consider myself guilty of a kind of *felo-de-se*, if I were not to step forward in the present case in my own vindication *."

Let us next hear his own opinion and eulogium of his own *style*. "I may be very often accused of great carelessness of style; but, *never*, unless I deceive myself egregiously, either of want of energy or diffuseness. *Indeed the characteristic properties of my style are just the opposite of diffuseness. I am remarkably concise, though I hope always clear, and generally energetic* †."

One would fancy on reading the above, that it was a fragment of some Academician's eulogy on the style of Tacitus or Montesquieu. But the passage itself is a good example of Doctor Thomson's incessant tautology. And by what splendid performance has he earned his privilege of being often guilty of great carelessness of style; and how is this, his acknowledged and habitual vice in writing, compatible with conciseness, clearness and energy?

Of the great versatility of Dr. Thomson's powers of expression, our readers may judge by the two following samples. If the first be reckoned diffuse, the second will be allowed to be more energetic, though it may be doubted how far he is entitled to credit for the vigour of its diction. "Such, as far as I am acquainted with them, are the changes produced by vegetation. *But plants do not continue to vegetate for ever*; sooner or later they *decay* and *wither* and *rot*, and are *totally decomposed*. This change, indeed, does not happen to all plants at the end of the same time Some live only for a *single* season, or even for a shorter period; others live *two* seasons; others *three;* others a *hundred* or more; and there are some plants which continue to vegetate for a *thousand* years; but *sooner* or *later* they all cease to live; and then these very chemical and mechanical powers, which had promoted vegetation, combine to destroy the remains of the plant. Now what is the cause of this change? *Why do plants die*? This question can only be answered by examining with some care what that is which constitutes the life of plants; for it is evident that if we can discover what that is which constitutes the life of a plant, it cannot be difficult to discover whatever constitutes its death. Now the phenomena of vegetable life are, in general, *vegetation*. As long as a plant continues to vegetate, we say that it lives; when it ceases to vegetate, we conclude that it is dead ‡." This mode of writing, so concise, clear and energetic

* Annals of Phil. April, 1822, p. 242. † Answer, ut supra, p. 245.

‡ System, Edition 6th, iv., p. 361. In quoting this before, we did injustice to our readers, by omitting the earlier portion of the passage.

in the author's mind, we ventured to denominate the *vacuous**, and gave numerous examples equally remarkable with the above. The Doctor is astonished at the inappropriateness of the epithet; and to prove that he can be concise, clear and energetic, he has lately favoured the world with the following document. "Having been led by very false information, to accuse most unjustly Thomas Allan, Esq., Banker, in Edinburgh, on a subject connected with his mineralogical pursuits, I now *publicly* express my sincere regret for having propagated a *most groundless calumny* against that gentleman, and do declare, that I now find that so far from what was reported to me, and repeated by me, having the slightest foundation, Mr. Allan, on the contrary, was the direct means of tracing and transmitting to the proper owner in London, the minerals which were the subject of the charge." (Signed) *Thomas Thomson.* Vide Constable's Edinburgh Magazine for March, 1822, p. 408, article entitled "Jury Court, Jan. 21, Allan *v.* Thomson—Apology for Defamation." "Mr. Jeffery (Advocate) said, he had only to regret, that this accommodation had not been entered into at an earlier stage, but that it had been delayed till the latest moment (the day of trial,) that such a measure was practicable; he added, that as the delay was to be imputed to Dr. Thomson, it was understood that gentleman should defray the whole expense incurred. This the Counsel for Dr. Thomson acquiesced in, it being stated by the Court that it was a matter of private arrangement†." Mr. Allan, as all our scientific readers know, is a gentleman distinguished by the excellence of his mineralogical collection, the beauty of its arrangement, and the complaisance with which he lays it open to men of science.

That our judgment concerning the general merits of Dr. Thomson's manner of systematizing chemistry, is in unison with that of the leading chemists of the age, we have abundant documents to prove. We shall content ourselves with a few extracts from the writings of Professor Berzelius, a gentleman whom the Doctor would wish to number in his list of friends, and against whose knowledge of the subject, and amiable temper, he can have nothing to object. In the Professor's paper on the composition of the oxides of platinum and gold, inserted in the *Annales de Chimie et de Physique*, for Oct. 1821, we observe the following exposure of Dr. Thomson's mode of perverting chemical results. "Dr. Thomson, however, in the 7th (6th) edition of his System of Chemistry, adopted the peroxide of Mr. Cooper, rejected that of which I had made the analysis, and of which I had described the combinations with the acids and alkalis, and *formed* on the analytical *data* of Mr. Cooper, a tritoxide of platinum in the following manner:—' Mr. Edmund

* See our Review *passim*. † Constable's Mag. *ut supra*.

Davy has found that 100 parts of platinum combine with 11.86 parts of oxygen; and on the other hand, M. Berzelius has determined that 100 parts of this metal combine with 16.494 parts of oxygen. The mean of the results of Edmund Davy and Berzelius, would be 14.177, for the oxygen in the peroxide; now this quantity does not differ much from 13.269, which represents the quantity of oxygen which would be necessary to form a tritoxide.' " *This method of treating the Science of Chemistry*," adds Mr. Berzelius, " *is peculiar to M. Thomson* *."

Well may the Swedish Philosopher say so. This travesty of experimental results is of a piece with that account of the chlorocyanic acid of M. Gay Lussac, which has excited so much ridicule among the French Chemists. Our Doctor actually describes as a liquid, what M. G. Lussac analyzed in the gaseous form; and he enjoins as the mode of making it, a method of verifying its nature after it is made. No man could prepare it by Dr. Thomson's misdirections.

The Swedish Chemist, in his remarks on the prussiates published in the 15th volume of the *Annales de Chimie et de Physique*, has the following passage: " This is the first example," says Dr. Thomson, " which does not agree with chemical theory; I therefore invite chemists to seek for its explanation; because nothing contributes so much to the advancement of science, as the development of what may appear to contradict opinions already received." To which quotation Berzelius subjoins, " he ought to have added, with the exception of those cases *where the contradiction is the result of an experiment ill-made and inexact, whereby the Science makes a very negative gain* †."

As the public have it in their power to compare at leisure our Review with the Regius Professor's Answer, we had resolved at first to leave the matter entirely in their hands, without further comment, perfectly tranquil as to the final decision.

* " Les expériences analytiques de M. Cooper, ainsi que les conclusions qu'il en a tirées, me parurent ne mériter aucune attention. Cependant M. Thomson, dans la septième édition de son *Système de Chimie,* adopta le protoxide de M. Cooper, rejeta celui dont j'avais fait l'analyse, et dont j'avais décrit les combinaisons avec les acides et les alcalis, et forma sur les données analytiques de M. Cooper, un tritoxide de platine de la manière suivante; M. Edmond Davy, a trouvé que 100 parties de platine se combinent avec 11.86 p. d'oxigène, et d'un autre côté M. Bèrzelius à determiné que 100 parties de ce métal, se combinent avec 16,494 part. d'oxigène. Le terme moyen des résultats d'Edmund Davy et de Berzelius serait 14.177 pour l'oxigène dans le peroxide; or ce terme moyen ne diffère pas beaucoup de 13.269, qui représente la quantité d'oxigène qui serait nécessaire pour former un tritoxide. Cette méthode de traiter la science est *particulière* à M. Thomson."—*Ann. de Chimie et de Phys.* Oct. 1821, p. 148.

† A quoi, ce me semble il auroit faller ajouter; à l'exception des cas où la contradiction est le resultat d'une experience màl faite et inexacte où la science fait un gain bien negatif. Ann. xv. 147.

But as we conceive some instruction may be derived from an exposure of his recent distortions of scientific truth, we think it our bounden duty as Journalists, to bestow on the Doctor a few parting admonitions, after which we shall leave him in the quiet enjoyment of his self-importance.

We had said in the Review, " We are at a loss to learn why a new edition has come forth; it was not spontaneously called for, and nothing but a decidedly superior work should have been tendered to the public." The Doctor makes answer: " The new Edition, I presume, was printed because the old had been sold. I am not aware that booksellers proceed in any other way *." Now the public should be informed, that during the session immediately preceding the appearance of his sixth edition, his fifth of 1817, was currently rattled off at the Edinburgh book auctions.

To multiply editions in this way, by the aid of the hammer, without the employment of the file, (*limæ labor*,) may suit a book-making factory, but is a great injustice to chemical students, and ought therefore to be exposed and checked. " The assertion," says he, " that the second volume is a repetition of the first, is so palpably untrue, that the Reviewer must have been aware of its inaccuracy when he made it †." Here we have his favourite figure of speech,—perversion of an author's sense. The Reviewer said, in discussing the merits of the second volume, " Before he reaches the combustible acids, one hundred and thirty-three pages are occupied with details, which, to a very great extent, had been already given in the first volume. His unsalifiable oxides are the two oxides of azote, the two of hydrogen, and carbonic oxide, which occupy five sections. These are for the most part repetitions of what he had given us before." Of these book-making repetitions we shall now give some samples.

" If equal volumes of chlorine and hydrogen be put into a glass tube, and exposed to the direct rays of the sun, an explosion takes place. This curious fact was first observed by Gay Lussac and Thenard. When two equal glass vessels, ground so as to fit each other, and filled, the one with dry chlorine, and the other with hydrogen, are placed in contact, and exposed to the light of day, but not to sunshine, the yellow colour gradually disappears, and the mixture becomes colourless. If it be now examined, it will be found converted into pure muriatic acid gas, equal in bulk to the volume of the two gases before combination. Hence it follows that this gas is a compound of chlorine and hydrogen." " Muriatic acid, called hydrochloric acid by Gay Lussac, is a gaseous body, invisible and elastic like common air, and having a peculiar smell, and a very sour taste. Water absorbs it with great avidity, so that it can be preserved

* Answer, p 244. † Answer, p. 245.

only over mercury. No *combustible* body will burn in it; and it destroys life instantly, when an attempt is made to breathe it." "Its specific gravity is the mean of that of chlorine and hydrogen, or 1.2847. Hence 100 cubic inches of it weigh 39.162. grains It constituents are as follows:

Hydrogen	0.125	......	1
Chlorine	4.5	......	36."

The above is found at pp. 221 and 222 of Vol. 1st. In volume 2d, we have the following, at page 247.

"When equal volumes of chlorine and hydrogen gas are mixed together, if the mixture be exposed to the direct rays of the sun, or if an electric spark be made to pass through it, an explosion takes place, the two gases combine without any change of bulk, and are converted into muriatic acid gas. Hence muriatic acid is a compound of equal volumes of the two gases, or it is composed by weight of

Hydrogen	0.125	....	1
Chlorine	4.5	....	36."

"Since the composition of muriatic acid has become known, Gay-Lussac has thought proper to give it the name of hydrochloric acid, in order to point out its composition." "It possesses the following properties. It is invisible, like common air, and capable, like it, of indefinite contraction and expansion. Its smell is peculiar. It reddens vegetable blues, and has a very sour taste. Its specific gravity is 1.2847, that of common air being 1. Animals are incapable of breathing it; and when plunged into jars filled with it, die instantaneously, in convulsions. Neither will any combustible burn in it. If a little water be let up into a jar, filled with this gas, the whole gas disappears in an instant, the mercury ascends, fills the jar, and pushes the water to the very top. Hence the necessity of making experiments with this gas over mercury." Vol. II. p. 243 and 244.

Let us look next to Ammonia. "This substance was unknown to the ancients. The method of obtaining it, is described by Basil Valentine. If this salt (carbonate of ammonia) or sal ammoniac, be mixed with *twice its weight* of quicklime, put into a flask, and exposed to the heat of a lamp, a gas comes over, which must be received over mercury, and which is ammoniacal gas. This gas was first discovered by Dr. Priestley." "Ammoniacal gas is transparent, and colourless, and possesses the mechanical properties of air. Its smell is very pungent, though rather agreeable when sufficiently diluted. Its taste is acrid and caustic, and if drawn into the mouth, *it corrodes the skin.* Animals cannot breathe it without death. When mixed with oxygen gas, and an electric spark passed through the mixture, it detonates as was first discovered by Dr. Henry. It

converts vegetable blues into green. Its specific gravity is 0.590. Water absorbs 780 times its bulk of this gas, and is converted into liquid ammonia. When this liquid is heated to 130°, the ammonia separates in the form of gas. When electric sparks are passed for a considerable time through dry ammoniacal gas, its bulk is just *doubled*, and it is completely decomposed. The new gaseous product consists of a mixture of three volumes of hydrogen gas, and one volume of azotic gas. It is obvious from this, that ammonia is composed of three volumes of hydrogen, and one volume of azote, compressed into two volumes. Hence its constituents by weight are—

Hydrogen	0.2082	0.125×3	1
Azote ..	0.9722	1.75	$4\frac{2}{3}$

Thus we see that ammonia is a compound of three atoms of hydrogen and one atom of azote. Hence the weight of an atom of it is 2.125"—Vol. 1. p. 224.

"It (ammonia) may be procured in the following manner; put into a retort a mixture of *three parts* of quick-lime, and *one part* of sal ammoniac in powder. Plunge the beak of the retort below the mouth of a glass jar, filled with mercury. Apply the heat of a lamp to the retort; a gas comes over which displaces the mercury, and fills the jar. This gas is ammonia. Ammonia was altogether unknown to the ancients. Basil Valentine describes the method of obtaining it." "Dr. Priestley discovered the method of obtaining it in a state of purity. Ammonia in the state of gas, is transparent and colourless like air; its taste is acrid and caustic, like that of the fixed alkalis, but not nearly so strong, *nor does it, like them, corrode those animal bodies to which it is applied*; its smell is remarkably pungent, though not unpleasant, when sufficiently diluted. Animals cannot breathe it without death. Its specific gravity is 0.590, that of common air being 1." "Water by my trials, is capable of absorbing 780 times its bulk of this gas." "The term *ammonia* almost always means this liquid solution of ammonia in water. When heated to the temperature of 130°, the ammonia separates under the form of gas." "When oxygen and ammoniacal gases are mixed, the mixture may be fired by an electric spark, as was first observed by Dr. Henry." "Dr. Priestly discovered that when electric explosions are made to pass through this gas, its bulk is gradually augmented to THRICE the space which it formerly occupied, and a quantity of hydrogen gas is produced." "Ammonia then is composed of three volumes of hydrogen, and one volume of azote, or which comes in this case to the same thing, of three atoms hydrogen, and one atom azote. Hence its constituents by weight are:

Hydrogen	$0.125 \times 3 = 0.375$	100
Azote	1.75	466.6

And the weight of an atom of ammonia is 2.125." Vol. II., p. 27., *et seq.* Should our readers wish for further examples of bare-faced repetition, they may compare the articles, azote, carbon, phosphorus, sulphur, hydrogen, *&c.*, in the first volume, with nitric and nitrous acids, phosphoric and phosphorous acids; sulphuric acid, muriatic acid, *&c.*, in the second.

In our Review, we said, that "azote, which had figured by itself as an *incombustible* in the first volume, becomes a *combustible* in the second." Let us hear his Answer on this head. "What pitiful quibbling is the Reviewer guilty of, in order to make out the combustibility of this substance." This exclamation of the Doctor's is diverting; for the quibbling is all his own, as the following quotation proves. "The only simple *combustibles*, as far as we know at present, capable of uniting with oxygen, and forming unsalifiable oxides, are AZOTE, hydrogen, and carbon*."

Of his *consistency*, the following is a fine example.

"Chap. II. Of *simple incombustibles*. By *incombustible*, I mean a body neither capable of *undergoing* combustion, nor of *supporting* combustion. It unites to all the supporters; but the union is never attended with the evolution of heat and light. We are at present acquainted only with one such substance, namely *azote*†." What a confined intellect must that man have who contradicts himself so flatly in different parts of the same book, revised for the sixth time; and who pours out a torrent of invective on the critic, who calls azote a combustible, *on his own authority.*

"As for Dr. Wollaston," says he, "the introduction of his name is most uncandid‡." We certainly think that it was most uncandid in the Doctor to introduce such a name, in such a way; for the introduction was not our doing, but his own, as the following quotation, from his *Annals*, many months before the Review was written, will shew. "The *methods* followed by Dr. Wollaston, Professor Berzelius, Mr. Dalton, and every other person, who has hitherto turned his attention to the atomic theory, are obviously not susceptible of any great degree of precision. They have been guided entirely by the analytical researches, without any general principle to direct their choice."

In his System, he says, "All acids, therefore, (with the exception of those above-named, sulphuretted hydrogen, telluretted hydrogen, and arsenuretted hydrogen) are combinations of supporters and combustibles.||" We ventured in the Review to ask "What is iodic acid? what is chloriodic acid? what is chloric acid? what is Count Von Stadion's perchloric acid? where is their combustible element? we do not find it among

* *Thomson's System*, 6th Ed. II. 3.
† System I., p. 202.
‡ Answer, p. 251.
|| II., 30.

any or all of your combustibles.*" These questions seem to have nettled the Doctor. Let him bring order out of his own confusion at his leisure. It is not our affair.

We are glad to see his recantation, however awkward, relative to his treatment of Sir H. Davy's electrical discoveries; the merit of which, however, is too eminent to be tarnished by the Doctor's breath, and too splendid to need the varnish of *his* stile.

Dr. Thomson has nineteen pages in his System, on combustion; the first sixteen of which we again assert are absolute and unmeaning *verbiage;* and in the remaining three, he has contrived to turn into "*caput mortuum*" of his own, one of the most interesting memoirs of modern science. Yet he has the assurance to say, "I have given, I conceive, all the important additions to our knowledge of flame contained in Davy's paper." If he will look into the article *combustion* in the dictionary of chemistry, published about the same time with his sixth edition, he will find a multitude of important facts, drawn from Sir H. Davy's Memoirs on Flame, of which there is no trace in his *all-perfect* System.

Under chlorine in his System, he says of M.M. Gay-Lussac and Thenard, "they shewed that the opinion, that oxymuriatic acid contains no oxygen, might be supported; but, at the same time, assigned their reasons for considering the old opinion as well founded. An abstract of these important experiments had been published however in 1809; these experiments *drew* the attention of Sir H. Davy to the subject." In our Review, the clearest evidence was adduced that Sir H. Davy's *attention* had been drawn to the subject of muriatic acid, long before these eminent French chemists had published their experiments. In his answer he perverts as usual the drift of our animadversions, to suit his private purpose. On this occasion he is driven to the necessity of citing his well-known sarcasm, against that accomplished and amiable philosopher, M. Gay-Lussac, which he vented in his Annals of Philosophy for January, 1816. It suited his temper *then,* to advocate Sir H. Davy's claims: for the controversy about the miner's safe-lamp, was yet *in embryo.*

From his answer, he appears to deny that the sun and fixed stars are masses of light, or that the cause of light is condensed in their discs; because in his System, he had affirmed of light, that "the third, and not the least singular of its peculiar properties, is, that its particles are never found cohering together, so as to form masses of any sensible magnitude." The atmosphere, electricity, and caloric, have also this *peculiar* property.

"I am accused of having perverted Davy's account of chloriodic acid, to suit my own atomic notions†." By accident, the detail of his perversion was left out of our Review. We here insert it from the original manuscript for the Dr's. edification.

* Journal of Science XI., 153. † Answer, p. 259.

"Operating in this way," says Sir H. Davy, "I find that iodine absorbs less than one third of its weight of chlorine." "In another experiment in which the sublimate was not dissolved by water, and in which the absorption was judged of, by the admission of fresh quantities of the gas, twenty grains of iodine, caused the disappearance of 9.6 cubical inches of chlorine.*" The mean of these two results gives very nearly one atom of chlorine to one atom of iodine, for the composition of chloriodic acid. Dr. Thomson omits, purposely it would appear, the statement above quoted, and brings forward another experiment, in which, from the admission of water, the same precision could not be obtained by that philosopher. He thus seeks to shew, that Sir H. Davy is in the wrong, relative to the atomic proportions, and that he himself is right, in pitching two atoms of chlorine against one of iodine.

The article *phosphoric acid* in his "Answer," contains an unfortunate anachronism which it will puzzle his veracity to reconcile. "Soon after this, I went to Glasgow (September or October, 1817), and I was not in possession of a laboratory for nearly two years. One of the first things which I did as soon as it was in my power, was to repeat Dalton's experiment (on phosphuretted hydrogen). I found it inaccurate. The whole structure immediately tumbled to the ground; and I was led back to the original opinion which I had stated in my paper on phosphuretted hydrogen gas. And those gentlemen who attended my lectures the ensuing course, will remember that I then gave the composition of phosphorous and phosphoric acids, precisely as in my sixth edition. Davy's paper appeared *soon after*, and confirmed me in the accuracy of my experiments.†" Dates are fatal to our Doctor's statement. He gives us here to understand, that nearly two years after his arrival in Glasgow, that is in the autumn of 1819, he was put in possession of a laboratory; and one of the first things he did in it, was to repeat Dalton's experiment. He adds "Davy's paper appeared soon *after*." We marvel at his venturing to publish so palpable a falsehood. For in his *Annals* for May, 1818, we have an account of Sir H. Davy's paper, on phosphorous and phosphoric acids, which had been read before the Royal Society on the preceding ninth of April.

The following is Thomson's notice, fifteen months before he was in possession of a laboratory; "In the hypophosphoric acid the proportions will be 45 phosphorus, to 15 oxygen .... (3. : 1.0)

In phosphorous acid ...... 45 to 30 (1.5 : 1.0)

And in phosphoric acid ...... 45 to 60 (1.5 : 2.)

"On some future occasion, we hope to give a more complete

* *Phil. Trans.* for 1814, part 2d.

† Answer p. 261.

account of this very valuable and elaborate paper, which we conceive to be one of the most perfect specimens of analytical research, that the author has produced*." This future occasion never arrived; but in his new edition Dr. Thomson assumes for himself the main part of the honour, which in the first moment of admiration, he here gives to the English philosopher. When he published the preceding abstract, he set up no claims of anticipation. He does not there say, as in the sixth edition of his System; " The analysis of phosphorous acid, given by Davy, in his paper published in the Philosophical Transactions for 1818, exactly agrees with mine, and serves, of course, to confirm it†." Now the Doctor's *last* decision, prior to Sir H. Davy's paper, is to be found in his fifth edition, published about six months before. Here he says, " It will appear in a subsequent part of this section, that phosphorous acid is composed of 1.5 phosphorus and two oxygen." " Thus the acids of phosphorus are composed as follows:

	Dr. Thomson. Phosph.	Oxygen."		Sir H. Davy. Phosph.	Oxygen.
Hypophosphoric acid	1.5 :	1	....	3 :	1
Phosphorous acid	1.5 :	2	....	1.5 :	1
Phosphoric acid	1.5 :	3‡"	..	1.5 :	2

Our readers may apply to the preceding sample of critical chicane, any epithet which they please.

With regard to the thermometer, we again affirm that his account of it is contemptible, for he gives no directions whatever, how this indispensible instrument of chemical research may be constructed, or how its indications may be verified. This is the more necessary, as the greater part of thermometers are very incorrectly made. The notice compatible with a *manual* for general students, is surely very different from what we are entitled to expect in a *system.* In lieu of printed information, the Professor invites us to attend his lectures on the subject at Glasgow, which invitation we have already given reason for declining. Indeed we had rather attend a Dutch prelection on Puffendorf.

In the number of Thomson's Annals published on the first of June, 1815, we find a copious abstract of Sir H. Davy's paper on the deutoxide of chlorine, read to the Royal Society on the preceding 4th of May. In Gilbert's *Annalen* for February following, that is eight months after Thomson's promulgation, and nine months after the above public reading, we have an account of what Dr. Thomson, in one page of his system calls " Count Von Stadion's deutoxide of chlorine, discovered *about the same time;*" but of which, he says with his usual inconsistency in the next page, " it differs so much from the gas examined by Davy, that it is probable they are distinct substances."

* *Thomson's Annals,* XI. 382. † *System,* 6th Ed. I. 263.

‡ *System* 5th Ed. I. 266.

In adverting to this subject, the Reviewer certainly did mean to arraign the Doctor's accuracy and candour for setting up pretensions to a discovery which was probably never made, and which, at all events, from its date could not affect the prior claim of the English philosopher.

Dr. Thomson confesses his blunder, pointed out by the Reviewer, in respect to fluorine; but softens it into an arithmetical error, sinking the long train of theoretical nonsense which he had tacked to its tail. The atomic number 1.25, which he now gives for fluoric acid, is very nearly that, deducible from an experiment long ago made by Sir H. Davy, on fluate of potash. "Twenty-two grains of fused sub-carbonate of potash, decomposed by diluted liquid fluoric acid, in an experiment made with great care, was found to afford 18.15 grains of dry fluate of potash *. But 22 : 8.75 : : 18.15 : 7.22; from which subtracting the atomic weight of potash 6, we have 1.22, to represent an atom of fluoric acid.

In Dr. Thomson's Answer, we find one instance of complaisance, which we shall try to repay. "I shall gratify," says he, "this laudable curiosity of the Reviewer. I ascertained the volume of oxygen gas in the olefiant gas, by means of nitrous gas, employing Dalton's formula for the purpose †." We are surprised that so doughty a disputant, should betray the secret of his errors in so silly a way; for *Dr. Thomson, in the 6th edition of his System, condemns Mr. Dalton's formula*, which, indeed, had been shewn long ago by M. Gay-Lussac, to be good for nothing. Describing Mr. Dalton's formula, Thomson says, "To 100 measures of air, add about 36 of nitrous gas; note the diminution of bulk, and multiply it by $\frac{7}{19}$; the product gives the bulk of oxygen in the air examined. *I have made many experiments on this subject; but have* NOT *obtained results agreeing with each other.* Sometimes the multiplier is $\frac{7}{19} = 0.3684$; while sometimes it is as high as $\frac{21}{50} = 0.42$ ‡." It was the French philosopher who first cleared up by his doctrine of volumes (so strangely resisted by Mr. Dalton,) all these perplexities; thus reducing the analysis of air, or gaseous mixtures containing oxygen, to great simplicity and precision. Dr. Thomson will find a full account of this important part of practical chemistry, in the Dictionary of Chemistry, Article, EUDIOMETER.

In our Review, p. 145, his ignorance of the mode of working simple equations in algebra is exposed, though he is perpetually involving the plainest cases of arithmetic in a mist of algebraic symbols, as pedants make unlucky quotations from languages they do not understand. The merest tyro in calculation, would have been chastised at school for committing the Doctor's

* *Phil. Trans.* 1814. † *Answer*, p 267.

‡ *System*, 6th Edit. III. p. 168.

blunder; yet, on that ludicrous bottom, he rested his refutation of experiments on the constitution of carburetted hydrogen, conducted with very considerable care. "The Reviewer," says he, "has misstated my reasoning in the Annals; but it is not worth while to put him right." This statement is altogether false, as the following quotation will prove. Here is the Doctor's equation from the Annals of Philosophy.

$$\text{“}\ x = \frac{B\,b + A a}{B + A}$$

In the present case, $A = 1 \quad . \ a = 0.9722$
$B = 0.66 \quad b = 0.0694$

$$\text{Consequently } x = \frac{0.66 \times 0.0694 + 0.9722}{1.66} = 0.86178.\text{”}$$

Now we affirm that the youngest school-boy will laugh at such a shallow algebraist: for taking his own formula and numbers; $x = 0.611$ and not 0.86178. We shall be curious to see what subterfuge will next be adopted, in order to escape from this dilemma.

We shall now quote a passage from our Review, to shew the Doctor's mode of reply. "The following sentence shews the Doctor's incompetency as an experimentalist. 'Unless the flask be very completely exhausted indeed of common air, combustion takes place, when the phosphuretted hydrogen is let into it.' 'This confession betrays poverty of invention, and ignorance of the methods previously practised in such cases. If the Doctor will consult Sir H. Davy's Bakerian Lecture on the Alkaline Metals, he will find this philosopher, filling his flask with hydrogen, and then exhausting that gas, in order to get rid entirely of the atmospheric oxygen." "I affirm," says he now, "that my method is susceptible of greater accuracy than that which the Reviewer insinuates, that I did not know*." His method, which is the old one, of comparing the weight of a flask filled first with air, and then with any gas, certainly does not remove the fallacy of which he himself complained in the above quotation, arising from the partial combustion of the phosphuretted hydrogen, by the residuary air; and, indeed, since *his* air-pump always leaves a notable residuum of common air in his flask, "his method" is totally inapplicable to phosphuretted hydrogen. We would advise him to fill his flask after the first exhaustion, with azote, and not till after a second, or, perhaps, a third operation, to admit the combustible gas.

But the boasting tone in which he talks of his superior skill in taking the specific gravity of gases, is finely contrasted with his total ignorance of the subject. Thus Dr. Thomson speaks of himself as a person "who has determined the specific gravity

* *Answer*, p. 268.

of more than 20 gases, with a degree of care and accuracy seldom equalled, *and never surpassed*.*"

Dr. Apjohn in his " remarks on the influence of moisture in modifying the specific gravity of the gases," inserted in the number of the Annals, for May, 1822, justly says of Dr. Thomson, " his mode of estimating the effect of moisture on the densities of the gases, appears to me altogether incorrect. The principle of the method adopted by Dr. Ure for the same purpose is true, but his number for steam being too high, his results are erroneous." Dr. Ure's number for the specific gravity of the vapour of water at 212°, is 0.625, and 0.624 is the number given by M. Biot, in his Traité de Physique, on the authority of M. Gay-Lussac †. Nothing is there said of temperature, though it appears that M. Gay-Lussac in his valuable researches, did give the relative densities of steam and air, for the boiling point of water at $\frac{10}{16}$, or 0.625. This is also the theoretical quantity, regarding two volumes of vapour of water, as consisting of two volumes of hydrogen 0.13888

1 of oxygen 1.11111

1.24999

one half of which sum = 0.625, is the specific gravity, air at 212° being 1.000. But as 1.375 parts of air at 212° become 1,05833 at 60°, we shall have the density of steam at 212° to that of air at 60° = $0.625 \times \frac{1.05833}{1.375} = 0.481$ and not 0.472, as adopted by Dr. Apjohn from Dr. Thomson.

We shall now give a tabular view of the different results.

Gases.	Sp. Gr. of Dry.	Specific Gravity of Moist. Thomson‡.	Ure§.	Apjohn.
Air,	1.0000 . .	0.9829	0.9934	0.9907
Oxygen,	1.1111 . .	1.0921	1.1020	1.0998
Azote,	0.9722 . .	0.9555	0·9660	0.9634
Chlorine,	2.5000 . .	2.4583	2.4677	2.4644
Hydrogen,	0.0694 . .	0.6839	0.0789	0.0761

Ure's and Apjohn's numbers do not differ much. Thomson's are ridiculously erroneous, and would vitiate every investigation on gaseous matter. The true quantities, computed from Ure's rule, are as follows:

Specific gravity of Gases standing over water at 60°.

Air	....	0.9909
Oxygen,	....	1.0995
Azote,	....	0.9636
Chlorine	....	2.4653
Hydrogen,	....	0.07648

* *Answer,* p. 268.

† *Tableau de la pesanteur specifique des gaz, et de quelques vapeurs.* Vol. I. p. 383.

‡ These quantities are taken, either from his late paper on the moisture of the gases, or computed from his formula.

§ Deduced from the numbers given at the end of the article, Gas —*Dictionary of Chemistry.*

"The ratio, therefore," says Dr. Apjohn, "of the dry gas to the expansion produced in it, by the moisture, is that of $\frac{p-f}{f}$: 1. Dr. Thomson, through some oversight, considers this as the ratio in volume of the dry gas to the *steam*, with which it is saturated, whereas the true ratio of these is that of $\frac{p-f}{p}$: 1; inasmuch as a given volume of gas at any temperature saturated with moisture, contains as much steam as could exist at that temperature in a vacuum of equal capacity*." In numbers, the first ratio is that of 0.983 to 1; the second is 56.7 to 1; and 0.983 : 56.7 :: 1 : 57.7; so that Thomson commits the *trifling* blunder, on a subject in which he conceives himself supreme master and proficient, of making the volume of vapour in a moist gas about $\frac{1}{60}$ of the true quantity.

"We see from these examples," says Thomson in this wonderful paper "*on the influence of humidity in modifying the specific gravity of the gases*," "that the specific gravity of air is diminished very nearly, by the volume of vapour mixed with it. And the *lower the temperature, the more nearly does this approach to accuracy;* because the specific gravity becomes always *less and less considerable.*" What ignorance of physics! "The densities of all the rest (except hydrogen,)" says Dr. Apjohn, are diminished by moisture, and the *more* as we *descend* on the scale of temperature; for as we descend, their specific gravities increase, and of course the difference between steam and the constant quantity 0.472. As an inference also from this result, I may remark, *that the rule so much insisted upon* of taking the specific gravities of the gases at a low temperature, is so far from being general, as to apply to hydrogen alone †." Now listen to the regius expounder of science. "I have little doubt that the specific gravity of hydrogen gas found by Berzelius and Dulong; namely, 0.0688, was a little too light, in consequence of the presence of the vapour of water in it ‡." This is much too absurd. The vapour of water is seven times denser than that of hydrogen, and must therefore always increase the specific gravity of that gas. What a pity that Dr. Apjohn published his exposure of the Doctor's notions so early. We should have been favoured ere long from his laboratory, with experimental determinations of the specific gravities of the gases, saturated with moisture, in which the results would all have been clipped and trimmed to his fantastic tabular numbers, like the grotesque shrubs in a Dutch garden.

* *Annals of Phil.* May 1822. p. 386. † *Annals of Phil.* p. 387.
‡ *Annals of Phil.* April, 1822.

Of sheer effrontery, the following is a good example. He is trying to palliate the absurdity of his claims, relative to sulphurous acid. "As for the experiments of Davy to which the Reviewer refers, as the first which established the true composition of sulphurous acid, I am not acquainted with them *." Not acquainted with them! They are quoted in the Review from pages 273 and 274, of the Elements of Chemical Philosophy. We shall here re-quote the passage for Dr. Thomson's information. "If the specific gravities of sulphurous acid gas and oxygen be compared, and the last subtracted from the first, it will appear that sulphurous acid consists nearly of equal parts of oxygen and sulphur by weight. In *several experiments in which I burned sulphur, procured from iron pyrites, out of the contact of air or moisture, in dry oxygen over mercury, I found that the volume of the oxygen was very little altered.*" "These are experiments," adds the Reviewer, "in which the world may confide. Dr. Thomson sinks them entirely, and refers merely to his own, on which nobody can rely †.

Under lithia, we find him re-asserting that 2.25, is the equivalent number deducible from the mean of the foreign experiments. Now, according to Gmelin, 100 parts of carbonate of lithia consist of 45.54 lithia, and 54.46 acid. But 54.46 : 275 : : 45.54 : 2.299, or 2.3 as we stated in the Review. Again, M. Vauquelin says, "that 100 parts of lithia contain 43.5 of oxygen, and 56.5 of metallic base; and 43.5 : 1 : : 56.5 : 1.299 for the atom of lithium, to which, adding 1 for oxygen, we have that of lithia = 2.299 as above.

He even defends his absurd method of preparing muriate of barytes, by pouring muriatic acid on the calcined sulphuret, instead of adding it to its filtered solution, on the score that the muriatic acid of commerce is never free from iron. Chemists who operate with pure acids (and the muriatic need never be contaminated with iron,) will do well not to vitiate their product, by following Dr. Thomson's mis-direction. We can readily understand why he affects to prefer Mr. Dalton's table of sulphuric acid, which at the specific gravity 1.8447 is, by his own shewing, $4\frac{1}{2}$ *per cent.* wrong, to Dr. Ure's, which, at the above density, must be right, since it agrees with his own number, and which, in the other points, is probably never in error to the amount of one-half per cent.

The Doctor is peculiarly incensed at our exposure of the bundle of errors in his paper on oxalic acid, inserted in the Philosophical Transactions for 1808. We shall quote the pas-

* *Answer*, p. 268. † *Review*, p. 149.

sage, as it is a good sample of his way of tempting readers into a belief that he is right, while he must know that he is wrong. "The allegation of the Reviewer, that my analysis of oxalic acid was inaccurate, because my oxalic acid still retained 32.5 *per cent.* of water, is disgracefully unjust. He must have known that my experiments were not made upon oxalic acid, but upon a dry oxalate. *No water existed in the acid as I employed it*, and therefore none was to be deducted. My near approach to truth in these experiments, notwithstanding the numerous difficulties attending my method, affords unequivocal evidence of the great care employed in the experiment *." The words we have put into Italics are false, and he knows them to be so, as the world shall also forthwith learn.

"Eighty-nine grains of well-dried oxalate of lime," says Dr. Thomson in the above memoir, "were exposed in a small retort, to a heat gradually raised to redness; the products were the following:—

	Grains
45.6 cubic inches of gas, weighing	14.8
Water	6.4
Residue in retort	62.4
	83.6
Loss	5.4
Total	89.0

"The 62.4 grains of residue in the retort were composed of

Lime	33.4 grains
Carbonic acid	26.4
Charcoal	2.6
	62.4

"Now it is clear that the 89 grains of oxalate of lime were composed of lime 33.4, acid 55.6 = 89. The acid was completely decomposed, and resolved into the following products:

Carbonic acid	33.1
Inflammable air	13.5
Water	6.4
Charcoal	2.6
	55.6

"Had the experiments been made upon 100 grains of oxalic acid, instead of 55.6, it is clear that the proportions would have been as follows:

Carbonic acid	59.53
Inflammable air	24.28
Water	11.51
Charcoal	4.68
	100.00

* *Answer*, p. 271.

"The result of the other experiments on oxalate of lime were nearly the same as the preceding. The following may be stated in round numbers, as the mean of the whole:

Oxalic acid is a compound of	Oxygen	64
	Carbon	32
	Hydrogen	4
		100 *"

We shall now shew that more errors are crowded into the above concluding statement of the Doctor's paper, than are to be found in any dozen volumes of the Royal Society's Transactions besides. It is now known from the concurring results of Dulong, Berzelius, and Dobereiner, to which some decisive ones obtained in this country will soon be added, that oxalic acid, such as it exists in oxalate of lead, contains no hydrogen †. Now as Dr. Thomson's contained 4 per cent. of hydrogen, we must allow it 32 of oxygen, being the quantity equivalent to its saturation, and of course 36 per cent. of water is the inevitable result—a quantity actually greater than the Reviewer assigned. Though he says in his answer, "*No water existed in the acid as I employed it*," yet we find in the very memoir under discussion $11\frac{1}{2}$ per cent. of water *openly* stated. But oxalate of lime is by no means necessarily an anhydrous compound. "L'oxalate de chaux," says Berzelius in his late memoir, "peut se combiner avec de l'eau en deux proportions, dont l'une est $C\,a\,O^2 + 2\,aq.$ and the other $C\,a\,O^2 + 4\,aq.$, et le plus souvent on les obtient melées ensemble‡."

The above anhydrous oxalate is 8.0, to which adding 2 atoms water = 2.25, we have the sum 10.25; whence such a salt contains in 100 parts 22 of water. Thomson's contained 4 of hydrogen, along with its saturating quantity of oxygen, constituting a sum = 36 *per cent.* Now this is exactly the quantity corresponding to 4 atoms of water; for 8 oxalate + 4.5 water = 12.5, and 12.5 : 4.5 :: 100 : 36. Now taking away these 36 parts of water, or 4 hydrogen and 32 oxygen, certainly foreign to the constitution of real oxalic acid, we have a remainder of 32 carbon, and 32 oxygen, for its composition as given in the above blundering memoir. This proportion transferred to atomic numbers is 3 atoms of oxygen to 4 atoms of carbon; whereas its true composition is 3 atoms of oxygen to 2 atoms of carbon. *Such was his near approach to truth*, or rather such was his affinity to error *in these experiments!* He makes the proportion per cent. of carbon to be 50, while it is only 33.3. "Now it is clear," said he, "that 89 grains of oxalate of lime, well dried, were composed of lime 33.4, acid 55.6." From the sixth edition of his System, "*it is clear*" that 89 grains of well-dried oxalate of lime are composed of lime 39 . acid 61. For 8.0 : 3.5 :: 89 : 39; or rather

* Phil. Trans. for 1807, p. 63.

† See Berzelius's paper on the muriates of platinum and gold. *Annales de Chimie et de Physique*, Oct. 1821.

‡ *Ibid.* p. 156.

38.9375. So erroneous is he in this most simple matter! In fine, nothing is clear from this memoir, except the elaborate stupidity of its author.

In the answer we have three vain pages on mineralogy, but Dr. Thomson is careful to suppress the *private* reason, why Professor Jameson's name is erased from his references at the bottom of the pages of his sixth edition, while the text is retained; why every reference to the Scotish Mineralogist is expunged; and why, though in the sixth edition of the System, he has retained, most servilely, the long chapter "Of Compound Rocks," amounting to 35 pages, borrowed in his fifth from Professor Jameson's Geonosy, he has now obliterated the following note acknowledging the obligation! "For to that important work (Jameson's Elements of Geonosy,) I earnestly recommend the attention of every mineralogist. The sketch in the text, though only a very short abridgment, is as detailed as is consistent with the nature of the present work, *I was indebted to Professor Jameson for the* WHOLE *of the materials out of which it was formed.*"

Under "Analysis of Minerals" in his "Answer," he says, "The Reviewer has pointed out one or two typographical errors, and invented as many more." This is a palpable falsehood. The blunders pointed out by the Reviewer, in Thomson's Chapter on the Analysis of Minerals, arise not from the carelessness of the printer, but the habitual inaccuracy of the author, which is here so redundant as to leave no room for invention on the Reviewer's part.

His assertion about liquid muriatic acid we had nearly passed over. We affirm that such experimental results as he gives, were never obtained; for they are impossible; and if he knew any thing of Chemical Arithmetic, he would not have offered them to the public. The demonstration is given in the Review, as clearly as numbers can make it. If he wants more evidence on this subject, he will find it in the last number but one, of this Journal.

With what decency or prudence can Dr. Thomson speak of the severity of our censure of Berzelius' mineralogy in an early number of this Journal, when *he himself* was the sole cause of that severity, by ushering before the English public a pretended translation of the Swedish work, so distorted and erroneous as to excite the grief and indignation of its celebrated author, and to lead him to lay a formal charge of something like literary fraud against Dr. Thomson, before the tribunal of European science*!

We have taken some pains to ascertain how far Dr. Thomson is correct in what he says, pages 253 and 254 of his *Answer*,

* See the complaint of Berzelius, in our Review of his Treatise on the Blow-Pipe, page 319 of this Number.

concerning Sir H. Davy and the atomic theory; and we are warranted in affirming, that his account of the matter is altogether false. Sir H. Davy has never embraced the atomic theory as taught by Mr. Dalton, or as expounded by the Regius Professor. With the first and great part of the doctrine of chemical proportions, that of reciprocal saturation of acids, bases, and salts, Sir H. was acquainted long before Mr. Dalton broached it. It was clearly taught by Richter; and a fine example of it is to be found in the Chemical Researches on Nitrous Oxide, bearing date, June 25, 1800. We know, at the present day, of no single specimen of chemical equivalents bletter developed than the following: "Hence," says Sir H., "we gain the following equation:—

"6.5 green sulph. of iron = 2.41 sulph. acid + 4.1 gr. ox. iron.
1.44 nitrous gas = 0.64 nitrogen + .8 oxygen.
1.0 water = 0.85 oxygen + 0.15 hydrogen.
Equal,
3.2 sulph. ammon. = 2.41 sulph. acid + 0.64 nitrog. + 0.15 hydr.
5.7 red ox. iron = 4.1 gr. ox. iron + 1.6 oxygen †."

Was the author of this equation, and of a multitude of similar researches, published so early as 1800, to get his first glimpse of the *atomic theory*, as Thomson miscalls it, or of the theory of reciprocal proportions, from any long conversation with Dr. Thomson, in 1807? How dare the Dr. say, therefore, "I knew from Davy himself that he had no idea whatever of definite proportions till Mr. Dalton had made known the outlines of his theory*." Mr. Dalton, through the courtesy of Sir H. Davy and his friends, was brought forward to lecture in the R. I. in 1803; at which time this ingenious observer thought he saw in Sir H. Davy's researches on the compounds of azote and oxygen, indications of multiple proportions, an idea manifestly thrown out and abandoned to the world by Mr. Higgins, a few years before. Sir H. Davy, with that integrity of intellect and attachment to the Baconian logic, which distinguishes all his inquiries, thought that Mr. Dalton strained experimental results to suit his theoretical opinions, and therefore hesitated to adopt the second part of what is now called the atomic theory, namely, the doctrine of multiple proportions, till experiments should demonstrate its truth. Mr. Dalton persevered most laudably to make out, by an extensive induction of facts, the justness of these views; which received their final confirmation from Dr. Wollaston's paper on the super-oxalic and sub-oxalic salts, and M. Gay-Lussac's beautiful memoir on gaseous volumes. At present, the theory of reciprocal and multiple proportions, as taught in Sir H. Davy's Elements of 1812, is undoubtedly a much truer representation of chemical principles than any thing

* Researches concerning nitrous oxide, p. 172.
† Answer, p. 254.

which Dr. Thomson has ever written, or can ever write on the subject.

We have now accompanied the Doctor through the various turnings of his tortuous course, and our readers must perceive how heavily his Answer has fallen back upon himself. Many of our most important criticisms he prudently shrunk from; but as these are fully before the public, we shall not fatigue our readers with references and repetitions.

Had Dr. Thomson confined his "Answer" to the statements contained in our "Review;" had he endeavoured, as he should have done, to have vindicated his character as a systematic writer by a calm and temperate reply, showing that our charges were exaggerated, and our conclusions erroneous, we should have left him in the quiet enjoyment of such refutation; but the tone and tenor of his answer are such as amply to confirm the justice of our criticism, and to seal his own condemnation; it is overbearing, petulant, and personal—full of topics entirely irrelevant, and evidently brought in with a view of drawing the reader's attention to any thing but the subject of our animadversions. In language much stronger than we should have any where used, Dr. Thomson tells his readers that we have accused him of being utterly incapable of writing English; of being ignorant of the first principles of arrangement; of having made many false statements to promote his own absurd and erroneous chemical speculations; and of having stuffed his book with innumerable errors, arising from his want of acquaintance with the elements of chemical science. These, we repeat, are Dr. Thomson's words; how far they are *true*, we now leave our readers to determine.

ART. XII.

ASTRONOMICAL AND NAUTICAL COLLECTIONS.

No. X.

i. *A Table to facilitate the calculation of the Equation to equal Altitudes, by* D. JOSEF SANCHES CERQUERO, *Lieutenant of a Frigate of the National Navy, in the Observatory of the City of Saint Ferdinand. Translated and communicated by* MR. ANDREW LIVINGSTONE.

Call the hour shown by the chronometer at the time of the first observation in the forenoon H, the corresponding apparent time h, the time which the chronometer is fast of apparent time a, the horary angle in parts of the equator P; and we shall have

$$H = h + a$$

$$h = 24^{h} - \frac{1}{15} P.$$

And consequently

$$H = 24^{h} + a - \frac{1}{15} P. \quad (A)$$

For the corresponding observation in the afternoon, indicating the quantities by the same letters, but with accents to distinguish them, and adding 24^{h} to H', that both observations may reckon from the same time, we shall have

$$24^{h} + H' = h' + a'$$

$$h' = \frac{1}{15} P'$$

And therefore

$$24^{h} + H' = a' + \frac{1}{15} P'. \quad (B)$$

The equations (A) and (B) give

$$\tfrac{1}{2} (a + a') = \tfrac{1}{2} (H + H') - \frac{\tfrac{1}{2} (P' - P)}{15}. \quad (C)$$

The quantity $\frac{1}{2} (a + a)$ is what the chronometer is *fast* of the precise middle time of the two observations, which is nearly the time of apparent noon; and the equation (C) shows that what it is fast is equal to the half sum of the two times H and H', *plus* a correction equal to half the difference of the two horary angles reduced to time.

The chronometer has been supposed *fast* of apparent time, and this may always be considered as the case, for if *slow* it can easily be read *fast* by merely subtracting what it is slow from 12^{h}.

Spherical Trigonometry gives

$$\tfrac{1}{2} (P' - P) = \frac{\tfrac{1}{2} x}{\text{sine } \tfrac{1}{2} (P' - P)}. \ \text{Tang. } L.$$

$$- \frac{\tfrac{1}{2} x}{\text{tang. } \tfrac{1}{2} (P' + P)}. \ \text{Tang. } D.$$

x being the sun's declinatory movement towards the North Pole in the interval betwixt the observations, L the latitude of the place, supposed North, and D the declination of the sun, also North, calculated for a time equidistant from both observations, which in the present case is the declination for apparent noon.

In this value of $\frac{1}{2} (P' - P)$, the quantities of the third order only have been neglected, being absolutely insensible, as has been shown by Delambre in his *Treatise on Astronomy*.

If the interval expressed in hours is called T, it is easy to see that $$15^\circ\, T = P + P'.$$

If the diurnal movement in declination is expressed by d D, it also follows that

$$24^{\text{h}} : T :: \mathrm{d}D : x = \frac{T}{24}.\ \mathrm{d}D$$

Substituting these values in the expression of $\frac{1}{2}(P' - P)$ and dividing by 15 to reduce it to time, it results that

$$\frac{\frac{1}{2}(P' - P)}{15} = \frac{\frac{1}{2}T}{360.\ \text{Sine}\left(15^\circ \frac{T}{2}\right)}.\ \mathrm{d}D\ \text{Tang. L}$$

$$- \frac{\frac{1}{2}T}{360.\ \text{Tang.}\left(15^\circ \frac{T}{2}\right)}.\ \mathrm{d}D\ \text{Tang.}\ D$$

$$= A\,\mathrm{d}D\ \text{Tang. L} - B\,\mathrm{d}D\ \text{Tang. D}$$

And in the last place, substituting in the equation (C)

$$\text{The hour of apparent time by the chronometer} = \tfrac{1}{2}(H + H) - A\,\mathrm{d}D\,\text{Tang.}\ L + B\,\mathrm{d}D\,\text{Tang.}\ D$$

The two last terms jointly form what is denominated the *equation to equal altitudes.*

If another corresponding altitude is observed the subsequent forenoon, and designated by the same letters, but with two accents, to distinguish the quantities appertaining to this third observation, we shall have

$$24^{\text{h}} + H' = a' + \frac{1}{15}P'$$

$$H'' = 24^{\text{h}} + a'' - \frac{1}{15}P''$$

And consequently

$$\tfrac{1}{2}(a' + a'') = \tfrac{1}{2}(H' + H'') + \frac{\frac{1}{2}(P'' - P')}{15}. \quad (E)$$

The quantity $\frac{1}{2}(a' + a'')$ will be the time that the chronometer was *fast* at midnight, or what is the same, the time of apparent midnight by the chronometer.

We shall also have in this case

$$\tfrac{1}{2}(P'' - P') = \frac{\frac{1}{2}x}{\text{Sine}\ \frac{1}{2}(P' + P'')}.\ \text{Tang.}\ L$$

$$- \frac{\frac{1}{2}x'}{\text{Tang.}\ \frac{1}{2}(P' + P'')}.\ \text{Tang.}\ D'$$

x' being the variation of the declination from the time of the afternoon until the time of the observation of the following forenoon, and D' the declination for midnight, or the time equidistant from both observations; calling the interval in time T, we have as before

$$x' = \frac{T}{24} \cdot \ \mathrm{d} D$$

$$(P' + P'') = 15° \, (24 - T')$$

$$\tfrac{1}{2} \, (P' + P'') = 180° - \left(15 \frac{T'}{2}\right).$$

Substituting these values in the expression of $\frac{1}{2} \, (P'' - P')$, keeping in mind that

$$\text{Sine } (180° - M) = \text{Sine } M$$

$$\text{Tang.} \, (180° - M) = - \text{Tang. } M,$$

and dividing by 15 to reduce it to time, it follows that

$$\frac{\frac{1}{2} \, (P'' - P')}{15} = \frac{\frac{1}{2} \, T'}{360 \text{ Sine } \left(15° \frac{T}{2}\right)} \, \mathrm{d} D \text{ Tang. } L$$

$$+ \frac{\frac{1}{2} \, T'}{360 \text{ Tang. } \left(15° \frac{T'}{2}\right)} \, \mathrm{d} D \text{ Tang. } D'$$

$$= A' \mathrm{d} D \text{ Tang. } L + B' \mathrm{d} D \text{ Tang. } D'$$

Finally substituting in the equation (E)

Time of apparent midnight by the chronometer $= \frac{1}{2} \, (H' + H'')$

$$+ A \, \mathrm{d} D \text{ Tang. } L$$

$$+ B \, \mathrm{d} D \text{ Tang. } D'$$

Comparing this expression with that which we have before found for noon, we see that they only differ in the sign of the first term of the correction. And thus, taking altitudes of the sun in the afternoon, and corresponding ones the following forenoon, we can find the time of midnight by the chronometer with equal facility, and by the same calculation as for noon, only changing the sign of the first term of the correction.

The interval T or T' ought strictly to be computed in apparent time; but good watches go so accurately, that 24 hours by the watch will be so nearly 24 hours of apparent time, that the interval shown by the watch may be considered as the interval of apparent time, without the least risque of any sensible error.

The table hereafter inserted gives the Logarithms of A and B to four places of figures; it is of single entry, the argument being the interval, " or elapsed time :" and for the whole calculation of the equation Logarithms to only four places of figures suffice.

Log. A + Log. dD + Log. Tang. L = Log. of the first part;

Log. B + Log. dD + Log. Tang. D = Log. of the second part.

Thus we see the whole operation reduced to finding the Logarithms A and B by the table, and three others to four places of figures by the ordinary Logarithmic Tables, of which every observer must be possessed.

In relation to the signs, the factor A is positive if the correction is required for midnight, and negative when it is wanted for noon: B is negative when the interval exceeds 12 hours; dD is negative when the declinatory movement is towards the south; Tang. D is negative if the declination is south; as is also Tang. L when the observer is in a southern latitude. On the right of each logarithm may be written the sign of the factor it represents, in the form shown in the examples.

With what has been said, and giving to each part the sign which results from the general rules of multiplication, the correction is obtained with more facility and accuracy united than by any other mode with which I am acquainted: in fact, the calculation is very short, symmetrical for both parts, the result exact to the tenth of a second in all cases which offer in practice, and answers for either the sun or the planets.

The declination of the sun must be taken from the *Nautical Almanac*, for noon or midnight, as may be required; but that of a planet, for the time of its superior or inferior passage.

Some may consider the attention to the *signs* of the factors necessary to show that of the product as troublesome, desiring some particular rule as better for assigning the manner in which each part of the equation ought to be applied. Persons of so bad a taste may avail themselves of the following rules when solar altitudes are observed; which will most frequently be the case.

The first part is additive when the sun's longitude is greater than 3^s and less than 9^s, otherwise it is subtractive. But the

reverse takes place in south latitude, or if the correction is required for midnight, unless both these circumstances concur.

The second part is additive when the sun's longitude is comprehended between 0^s and 3^s, or between 6^s and 9^s; otherwise subtractive. But this application changes if the interval exceeds 12 hours.

Example. On the afternoon of the 17th of September, 1810, altitudes of the sun were observed at Marseilles; and equal ones on the forenoon of the 18th: the interval was $21^h\ 50^m$; the latitude of Marseilles 43° 17′ 50″ N.: the "*Connaissance des Tems*," gives the sun's declination for midnight of the 17th at Marseilles as 2° 14′ 23″ N.; the diurnal declinatory movement d*D* being — 23′ 14″ = — 1394″. With these data the equation to equal altitudes is required.

Log. *A* (Table)	9.0349 +	
Log. Tang. *L*	9.9742 +	
Log. 1394″	3.1443 —	
Log of 1st part	2.1534 —	142″,37 first part.
Log. *B* (Table)	9 0172 —	
Log. Tang. *D*	8.5923 +	
Log. 1394″	3.1443 —	
Log. of 2d part	0.7538 +	5,″67 second part.

Equation for midnight — 136″,70 = $2^m\ 16^s$,70.

Former tables which give the equation for noon, and which are to be found in Lalande's Astronomy, Mendoza's Collection, and several of the Spanish Nautical Almanacs, have all of them the following defects:—1st, they are of double entry, and consequently both complicated and troublesome in use, exacting double, and even triple, proportional parts: 2d, they are constructed for short intervals, and consequently "in the majority of cases" cannot give the correction for midnight: 3d, they suppose the perihelium of the earth, and the obliquity of the ecliptic, constant, and it is necessary if accuracy is required to renew them from time to time: 4th, they are of no use for planets.

Baron Zach, in 1812, published at Marseilles, at the end of his particular tables of aberration and nutation, others for the present object which we are engaged in; they are free from the 1st and 2d objections, but the 3d and 4th still remain in his

XIXth table. To deduce the equation of Altitudes from these tables, the author finds two auxiliary arcs α and β, and two numbers a and b; and afterwards has to seek five logarithms to seven places of figures, and even then in the preceding example which he has computed in his introduction, he does not arrive at the exactitude of the tenth of a second. In fact, he himself says, that using his table XIX, he found "for the correction $-2^m\ 16^s,77$."

It is true he proposes another method accurate in its results, and applies it himself to the same example; but by this mode the same two auxiliary arcs α and β must be found, and also seven different logarithms to seven places of figures, "although two of them are constant," to obtain the solution $-2^m\ 16^s,67$, which is the same we have found with so much greater facility with merely the unimportant difference of $0^s,03$.

On this occasion we ought to notice some ambiguities which Baron Zach has overlooked in the application of his tables to the correction of the transit of a planet, determined by means of equal altitudes. At Florence, he says, Mars was observed on 8th April, 1809; the interval being $= 8^h\ 20^m$; dD $= +6'\ 38''$; latitude $= 43°\ 46'\ 40''$ N.; declination $= 5°\ 9'\ 40''$ S; required the correction for the planet's superior passage?

By our method we shall have

Log. A (Table)	8.1155 −	
Log. Tang. L	9.9815 +	
Log. 398″	2.5999 +	
Log. of 1st part	0.6969 — 4″,98	first part.
Log. B (Table)	7.7799 +	
Log. Tang. D	8.9558 −	
Log. 398″	2.5999 +	
Log. of 2d part	9.3356 − 0″,22	second part

Equation − 5″,20

Baron Zach finds, First part + 4″,97
Second part − 0″,22

Equation . . . . + 4″,75

Re-examining his calculation (page 36 of the prefatory part of his little work already cited) and correcting the slight oversight in taking out the number corresponding to the Log. 0.6968897, we find for the first part + 4″,98. Correcting also

for having given the sign + to the Tang. of the declination in the calculation of the second part when it ought to bear the sign — in consequence of the declination being *South*, we find or the second part + 0″,22, and the total equation + 5″,20, the same that we have deduced above, but with the contrary sign; it thus appears that independently of the oversight peculiar to this example, Baron Zach has changed the sign of the whole equation.

To make this more perceptible, let us calculate the other example proposed in the same page. At Pisa equal altitudes of Venus were taken on the 23d of March, 1809; the interval being $= 8^h\ 50^m$; latitude 43° 43′ 11″ N., declination = 20° 42′ 40″ N.; and dD = + 20′ 5″, or 1205″.

Log. A (Table)	8.1272 —	
Log. Tang. L	9.9806 +	
Log. 1205′	3.0810 +	
Log. 1st part	1.1888 —	15″,45 First part.
Log. B (Table)	7.7322 +	
Log. Tang. D	9.5776 +	
Log. 1205″	3.0810 +	
Log. 2d part	0.3908 +	2″,46 Second part.

Equation . . . — 12″,99
According to Zach + 12″,98

It is thus seen that using Zach's rules the sign is equivocal for the passage of planets; this, in my opinion, arises from that celebrated Astronomer's having changed (without doubt from inattention) the fundamental formula for the correction of the superior passage. In effect that formula is as we have already seen.

$$\text{Equation of altitudes} = -\frac{\frac{1}{2}T}{\text{Sine}\left(15°\frac{T}{2}\right)}\cdot\frac{1}{360}\cdot \mathrm{d}D\ \text{Tang.}\ L$$

$$+\frac{\frac{1}{2}T}{\text{Tang.}\left(15°\frac{T}{2}\right)}\cdot\frac{1}{360}\cdot \mathrm{d}D\ \text{Tang.}\ D$$

Zach exhibits this in his work before mentioned (page 30,) with the signs reversed.

Mr. Delambre has also mistaken the signs of the analytical expression of the equation for midnight, or the inferior passage, in

his *Compendium of Astronomy*, (page 223,) and in his *Astronomy*, (vol. 1st, page 560.) This may easily be seen by comparing the said expression as found in the two Treatises just cited, with what we have above deduced, or with that which the author himself has stated to be correct in the prefatory part of his Solar tables, published by the (French) Board of Longitude in 1806.

Such mistakes, and made by such respectable authors, have obliged me, at the beginning of this memoir, to occupy myself in giving demonstrations of both formulas, in order to show, beyond a doubt, which are the correct expressions.

The same Mr. Delambre, in the first volume of his Astronomy, has given tables for the equation of altitudes; by which five logarithms are to be found, as in our method, with only this difference that four of them are found in his tables ; it is however necessary to have recourse to the ordinary logarithmic tables for the tangent of the latitude; and to find the two parts of the equation by means of his logarithms : besides, four proportional parts are almost always required for the four logarithms which his tables furnish, while by our method no proportionals are required ; "except, perhaps, for the logarithms *A* and *B* :" and in finding the log. tangents of the latitude and declination, the nearest minute may be used, without danger of any sensible error in the result, from neglecting the odd seconds. I believe, therefore, that if no disadvantage attends Delambre's mode, it at least has no advantage over our method in point of brevity. In regard to accuracy, the 1st table has the 3d and 4th defects of the general ones of double entry which have hitherto been in use, defects common to all the class of which the sun's longitude is the argument. In the case of the first example we have given, with the said longitude for midnight of the 17th, at Marseilles, (174° 22′) the tables we speak of give $2^m\ 16^s,9$ for the equation of equal altitudes, differing from that found above by Zach's accurate method, and by our own, by $0^s,2$; a quantity which Astronomers will not disregard without some reluctance ; to this may be added that the differences in the said 1st table are considerable in some cases, and the interpolation is consequently very troublesome.

Those who, nevertheless, prefer Delambre's table, ought to

keep in mind that there is a mistake of the sign in the 1st table, page 578, column 4th. The whole of that column which gives the log. $\left(\frac{\mathrm{d}\,D\ \text{tang.}\ D}{360}\right)$ from 180° to 225° of the sun's longitude ought to have the sign + in place of − which has there been assigned to it either through the inattention of the celebrated author, or from an error of the press. There is also another mistake in the 2d table in the log. $\left(\frac{t}{\text{tang.}\ (15^\circ\ \ t)}\right)$ corresponding to $8^h\ 20^m$ of half-intervals, and which ought to be 0.7660 instead of 0.7460.

Finally, we cannot do less than recommend to astronomers and travellers, who are desirous of regulating their timekeepers by equal altitudes taken on two or more consecutive days, the advantage which results, without any increase of trouble, from making all the observations corresponding ones; that is, if in the forenoon and afternoon of the same day equal altitudes are obtained, it is very desirable that those of the subsequent day should not only be equal between themselves, but also equal to those of the preceding day; and also that the same should be the case on the subsequent days. Proceeding on with this care, the time of noon and midnight may be accurately found, and consequently the exact rate of the chronometer from 12 to 12 hours, thus establishing confidence in any observation made by that timekeeper during the interval from one *rating* to another.

It is also useful to take altitudes in the afternoon when clouds have prevented them from being observed the preceding forenoon, and with these and corresponding ones next forenoon the chronometer's absolute rate for midnight may be found; this is still more requisite if any observation is to be made by that chronometer during the same night, for it is interesting to obtain a rate for the timekeeper which will assure the apparent or mean time as near as possible to the time as shown by it when any phenomenon takes place.

It appears that many foreign Astronomers practise this method, and thus my advice is principally addressed to my countrymen, among whom I have seen none use it, notwithstanding its facility and utility.

A TABLE, intended to facilitate the calculation of the EQUATION to equal Altitudes. Argument = T = interval.

Arg. h m	Log. A ∓	Diff.	Log. B +	Diff.	Arg. h m	Log. A ∓	Diff.	Log B +	Diff.
2 00	8.0307	+ 9	8.0157	− 18	7 00	8.0883	+ 31	7.8728	− 95
10	8.0316	9	8.0139	20	10	8.0914	32	7.8633	101
20	8.0325	10	8.0119	21	20	8.0946	33	7.8532	106
30	8.0335	11	8.0098	22	30	8.0979	34	7.8426	112
40	8.0346	11	8.0076	24	40	8.1013	34	7.8314	118
50	8.0357	12	8.0052	26	50	8.1047	35	7.8196	125
3 00	8.0369	13	8.0026	28	8 00	8.1082	36	7.8071	132
10	8.0382	14	7.9998	29	10	8.1118	37	7.7939	140
20	8.0396	14	7.9969	31	20	8.1155	38	7.7799	149
30	8.0410	15	7.9938	33	30	8.1193	39	7.7650	159
40	8.0425	16	7.9905	35	40	8.1232	40	7.7491	169
50	8.0441	17	7.9870	37	50	8.1272	41	7,7322	181
4 00	8.0458	17	7.9833	39	9 00	8.1313	42	7.7141	194
10	8.0475	18	7.9794	41	10	8.1355	43	7.6947	209
20	8.0493	19	7.9753	43	20	8.1398	43	7.6738	226
30	8.0512	19	7.9710	45	30	8.1441	44	7.6512	245
40	8.0531	20	7.9665	48	40	8.1485	45	7.6267	267
50	8.0551	21	7.9617	50	50	8.1530	46	7.6000	293
5 00	8.0572	22	7.9567	53	10 00	8.1576	47	7.5707	322
10	8.0594	22	7.9514	56	10	8.1623	49	7.5385	358
20	8.0616	23	7.9458	58	20	8.1672	50	7.5027	402
30	8.0639	24	7.9400	61	30	8.1722	51	7.4625	455
40	8.0663	25	7.9339	64	40	8.1773	52	7.4170	523
50	8.0688	26	7.9275	67	50	8.1825	53	7.3647	612
6 00	8.0714	26	7.9208	70	11 00	8.1878	54	7.3035	734
10	8.0740	27	7.9138	74	10	8.1932	55	7.2301	911
20	8.0767	28	7.9064	78	20	8.1987	56	7.1390	1191
30	8.0795	29	7.8986	82	30	8.2043	57	7.0199	1702
40	8.0824	29	7.8904	86	40	8.2100	59	6.8497	2951
50	8.0853	30	7.8818	90	50	8.2159	60	6.5546	. . .
7 00	8.0883		7.8728		12 00	8.2219		. . .	

Arg.	Log. A ∓	Diff.	Log. B —	Diff.	Arg.	Log. A ∓	Diff.	Log. B —	Diff.
m					h m				
12 00	8.2219	+ 61	. . .	. . .	17 00	8.4737	+ 116	8.2581	+ 237
10	8.2280	62	6.5667	+3071	10	8.4853	120	8.2818	235
20	8.2342	63	6.8738	1823	20	8.4973	123	8.3053	234
30	8.2405	65	7.0561	1312	30	8.5096	126	8.3287	232
40	8.2470	66	7.1873	1032	40	8.5222	129	8.3519	231
50	8.2536	67	7.2905	855	50	8.5351	133	8.3750	229
13 00	8.2603	69	7.3760	734	18 00	8.5484	137	8.3979	230
10	8.2672	70	7.4494	645	10	8.5621	141	8.4209	230
20	8.2742	72	7.5139	578	20	8.5762	145	8.4439	230
30	8.2814	74	7.5717	524	30	8.5907	150	8.4669	230
40	8.2888	75	7.6241	481	40	8.6057	154	8.4899	232
50	8.2963	76	7.6722	447	50	8.6211	159	8.5131	233
14 00	8.3039	78	7.7169	416	19 00	8.6370	164	8.5364	236
10	8.3117	79	7.7585	392	10	8.6534	170	8.5600	238
20	8.3196	81	7.7977	371	20	8.6604	176	8.5838	240
30	8.3277	83	7.8348	353	30	8.6880	182	8.6078	244
40	8.3360	85	7.8701	336	40	8.7062	189	8.6322	248
50	8.3445	87	7.9037	323	50	8.7251	196	8.6570	253
15 00	8.3532	88	7.9360	310	20 00	8.7447	205	8.6823	257
10	8.3620	90	7.9670	300	10	8.7652	213	8.7080	263
20	8.3710	92	7.9970	290	20	8.7865	222	8.7343	271
30	8.3802	95	8.0260	281	30	8.8087	233	8.7614	279
40	8.3897	97	8.0541	274	40	8.8320	244	8.7893	287
50	8.3994	99	8.0815	267	50	8.8564	257	8.8180	297
16 00	8.4093	101	8.1082	261	21 00	8.8821	270	8.8477	308
10	8.4194	103	8.1343	256	10	8.9091	286	8.8785	322
20	8.4297	106	8.1599	251	20	8.9377	303	8.9107	336
30	8.4403	109	8.1850	247	30	8.9680	323	8.9443	354
40	8.4512	111	8.2097	244	40	9.0003	346	8.9797	375
50	8.4623	114	8.2341	240	50	9.0349	372	9.0172	398
17 00	8.4737		8.2581		22 00	9.0721		9.0570	

The factor A is *negative* when the correction is required for noon; but *positive* if it is wanted for midnight.

In like manner, treating of a planet, the factor A is *negative* if the correction is required for its superior passage, but *positive* if wanted for its inferior passage.

The factor B is *positive* when the interval is under 12 hours, and *negative* when the interval is greater.

This is what the signs affixed to the logarithms in the table indicate. In the logarithmic differences it may readily be seen that the signs merely indicate whether the logarithms are increasing or diminishing, and are entirely independent of the signs of their columns.

When the argument falls betwixt two of the times in the table, the use of the simple proportional is sufficient, and the magnitude and inequality of the differences, in some instances, need cause no uneasiness. The log. B, for example, varies unequally and rapidly near 9^h, 12^h, 14^h, and 21^h. Let us suppose, therefore, $dD = + 17' \, 10''$, $D = 17^\circ \, 2'$, these values produce a maximum in the value of dD tang. D for the sun. Notwithstanding this, the error from using the simple proportional will not be so much as $0^s,02$.

If the interval falls betwixt $11^h \, 50^m$ and 12^h, or betwixt 12^h and $12^h \, 10^m$, the second part of the equation is calculated in the first case for $11^h \, 50^m$, and in the second for $12^h \, 10^m$: the result will always be a very small quantity; with the knowledge of this, and that with an interval of 12^h the second part is $= 0$, it will be most easy to find the second part by the given interval. It may be noticed that the same precaution is required for this rare case in using Delambre's tables.

In relation to the first part of the equation, the use of the simple proportional produces only an error of $0^s,05$, with an interval of $20^h \, 6^m$ in the latitude of 60°, dD being $= 23' \, 40''$, which is the maximum for the sun. If the interval were greater, it would be a proof that the altitudes had been taken with less than two hours for a horary angle, and this might produce much greater uncertainty in the result of the observation, than any

error which could arise from the use of the simple proportional. This error, even with $21^h\ 56^m$ of interval with the same latitude, and the utmost value of d D, would amount only to $\frac{1}{4}$ of a second. But as such circumstances are very unfavourable for the determination of the time, they do not offer themselves to us in practice, and it follows that in cases which really occur, the use of the table, taking the simple proportional, is accurate to the tenth have asserted in the preceding memoir.

San Fernando, 1st May, 1820.

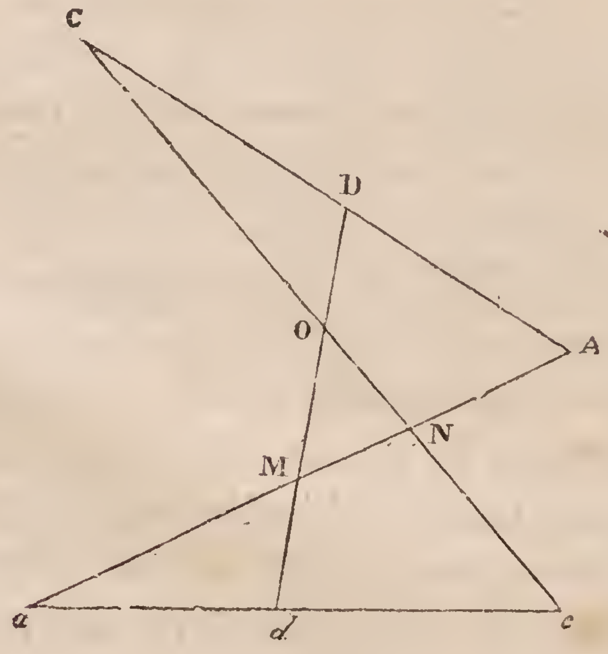
Translation finished at Liverpool,
24th December, 1821.
A. L.

ii. *An Essay on the easiest and most convenient method of Calculating the Orbit of a Comet from Observation. By* William Olbers, M D., 8vo. *Weinar.*

(Continued from Vol. XII. page 151.)

§. 58.

Supposing now *adc* to be the projection of the chord of the earth's orbit on the plane perpendicular to the middle position of the revolving radius for the earth; and *a*A, *d*D, *c*C, projections of the lines of direction on the same plane,



we have $CO : AM = \frac{CD}{\sin COD} : \frac{AD}{\sin DMA}$, and $cO : aM = \frac{cd}{\sin COD} : \frac{ad}{\sin DMA}$; consequently $CO + cO = \delta''' = \left(\frac{CD \,.\, AM}{DA} + \frac{a \,.\, M \,.\, cd}{ad}\right) \frac{\sin DMA}{\sin COD}$. Now calling $aM = f$ since $Aa = \delta'$, $AM = \delta' - f$; we have also, as in §. 38, $DMA = b'' - b'$, $COD = b''' - b''$; and the equation becomes $\delta'' = \frac{\sin.(b'' - b')}{\sin.(b''' - b'')} \left(\frac{DC}{DA}(\delta' - f) + \frac{dc}{ad} f\right)$. Again, if we put $\frac{DC}{DA} = \frac{t''}{t'} + p$, and $\frac{cd}{ad} = \frac{t''}{t'} + q$, we have, §. 57, $p = \frac{r''' \sin.\ \sigma}{r' \sin.\ \tau} - \frac{t''}{t}$, and $q = \frac{R''' \sin.\ (A''' - A'')}{R' \sin.\ (A'' - A')} - \frac{t''}{t'}$ but $\delta''' = \frac{\sin.\ (b'' - b')}{\sin.(b''' - b'')} \left(\frac{t''}{t'}\delta' + p\delta' - pf + qf\right)$; and since, § 38, $\frac{\sin.\ (b'' - b')\ t''}{\sin.\ (b''' - b'')\ t'} = N$, we have $\delta''' = N\left(1 + \frac{t'}{t''} p\right)\delta' + \frac{(q - p)\ f\ \sin.\ (b'' - b')}{\sin.\ (b''' - b'')}$.

Now we had, §. 38, $\varrho' = \frac{\delta' \cos.\ b'}{\sin.(A'' - \alpha')}$ and $\varrho''' = \frac{\delta''' \cos.\ b'''}{\sin.(A'' - \alpha''')}$, consequently $\left[\text{since } M\varrho' = \frac{N\delta' \cos.\ b'}{\sin.\ (A'' - \alpha''')}\right]$, $\varrho''' = M\left(1 + \frac{t'}{t''}\right) \varrho' + \frac{(q-p) \sin(b'' - b') \cos b''' f}{\sin(b''' - b'') \sin(A'' - \alpha''')}$; but $f = \frac{ad \sin b''}{\sin\ (b'' - b')} = \frac{R' \sin(A'' - A') \sin b''}{\sin\ (b'' - b)}$, and if we substitute this value of f in the second part of the value of ϱ''', it will become $h = \frac{R' \sin.\ (A'' - A')\ (q - p)\ \text{tang}\ b''}{(\text{tang}\ b''' - \text{tang}\ b'') \sin(A'' - \alpha''')}$, [since $\sin\ (b''' - b'') = \sin b''' \cos b'' - \sin b'' \cos b'''$, which, divided by $\cos b'' \cos b'''$, gives $\text{tang}\ b''' - \text{tang}\ b''$]; and substituting the values of tang b'' and tang b''', we have $h = \frac{R' \sin\ (A'' - A')\ (q - p)\ \text{tang}\ \beta''}{\text{tang}\beta''' \sin(A'' - \alpha'') - \text{tang}\ \beta'' \sin(A'' - \alpha''')}$ $= \frac{R' \sin\ (A'' - A')\ (q - p)\ m}{\text{tang}\ \beta''' - m \sin\ (A'' - \alpha''')}$; so that the denominator is the

same that has been already employed for M, §. 38; and the whole equation becomes

$$\varrho''' = M\left(1 + \frac{t'}{t''}p\right)\varrho' + \frac{R' \sin(A'' - A')(q - p)m}{\tang \beta''' - m \sin(A'' - \alpha''')}.$$

§ 59.

Hence we have the values of v and h in the equation $\varrho''' = (M+v)\varrho' + h$, which determine the influence of p and q, which were before neglected, on the value of ϱ'''; and the equation might be applied to the correction of the elements already calculated. But the labour may be much abridged by observing that the ϱ' already found can differ but very little from the true value which is now to be found; and if we call the approximate value (ϱ), we shall have, since h is also small, $\frac{h\varrho'}{(\varrho)} = h$, and the equation for ϱ''' will become $\varrho''' = M\left(1 + \frac{t'}{t''}p + \frac{h}{(\varrho)}\right)\varrho'$ and hence $\varrho''' = (M + v)\varrho'$, v being $= \frac{Mt'}{t'}p + \frac{h}{(\varrho)}$.

§. 60.

In order, therefore, to correct the two equations for r''' and k''', we must multiply all the coefficients which contain M by $\frac{M + v}{M} = H$, and those which contain M^2 by H^2. The equation for r' remains unaltered: and since the logarithms of the coefficients are already found, the operation is by no means difficult.

§. 61.

It will be convenient in the mean time to collect the equations for the determination of H together, for the sake of employing them more readily. When we have found (ϱ), which is the approximate value of ϱ', the time and distance at the perihelium, and ψ the true anomaly at the middle observation, as well as the differences of the anomalies τ and σ, we must compute

$$p = \frac{r''' \sin \sigma}{r' \sin \tau} - \frac{t''}{t'}$$

$$q = \frac{R''' \sin (A''' - A'')}{R' \sin (A'' - A')} - \frac{t''}{t'}, \text{ and}$$

$$h = \frac{R' \sin. (A'' - A')(q - p) m}{\text{tang } \beta''' - m \sin (A'' - \alpha''')}; \text{ and then}$$

$$H = 1 + \frac{t'}{t''} p + \frac{h}{(\varrho) M}.$$

But since in the last member of the equation for H, the h is divided by M, the expressions for these quantities having nearly the same denominators, we may take at once $\frac{h}{(\varrho) M} =$ $\frac{R' \sin (A'' - A')(q - p) m t'}{(\varrho)(m \sin (A'' - \alpha') - \text{tang } \beta') t''}$, and with this value of H we may proceed to correct the coefficients. We shall then obtain two new equations for r'' and k'' very little different from the former, from which the corrected value of ϱ' will be found so much the more easily, as its limits are so nearly ascertained by the previous determination of (ϱ). Two new hypotheses for ϱ', and an easy interpolation, will be fully sufficient for the purpose.

§. 62.

In order to illustrate the mode of computation still more completely, I shall return to the example of the comet of 1769, §. 46, 47. We have already found (§. 51) $\psi = 138°.19'.55''$, and we had $\varphi = 135°.52'.24''$, $\chi = 4°.27'.46''$, consequently $\sigma = 2°.27'.31''$, $\tau = 2°.0'.15''$; we had also $r' = 1.02367$, and $r''' = 0.83504$; consequently for p we have

Log r'	0.010160	Log r'''	9.921707
+ Log sin τ	8.543722	+ Log sin σ	8.632433
	8.553882	= L (r''' sin σ)	8.554140
		− L (r' sin τ)	8.553882
		= L. 1000.60	0.000258

And in this case $\frac{t''}{t'}$ being $= 1$, we have $p = .00060$.

For q we have $A'' - A' = 3°.53'.26''$, and $A''' - A'' = 3°.53'.49''$, hence

Log R'	0.003132	Log R'''	0.002184
+ Log sin$(A''-A')$	8.831555	+ Log sin $(A'''-A'')$	8.832267
	8.834687	L $(R''' \sin [A'''-A''])$	8.834451
		$-$L $(R' \sin [A''-A'])$	8.834687
		Log .99946	9.999764

consequently $q = -.000511$. Now in order to find $\frac{h}{(\varrho)M}$, we have $(q-p) = -.00114$. Hence

Log $R' \sin (A''-A')$	8.834687
Log $(q-p)$ [$-$]	7.056905
Log m	9.648938
	5.540530
$-$ Log .12210, the denom. §. 46	9.086716
$-$ Log (ϱ)	9.541829
Log $-$ 0 .00082	6.911985

We have therefore $H = 1 + p\frac{t'}{t''} + \frac{h}{(\varrho)M} = .99978$, and Log $H = 9.999904$; and in order to obtain the corrected coefficients in the equations for r''' and k'', we have only to subtract 96 from the logarithms of the terms containing M, and 192 from those which contain M^2: they will then become

$$r''' = \sqrt{(1.01011 - 1.21455\varrho' + .90829\varrho'^2)}$$
$$k'' = \sqrt{(.01868 - .10958\varrho' + .49694\varrho'^2)}$$

These equations differ so little from the former, that it is not worth while to recalculate ϱ' from them, especially as the calculation would only be a repetition of the former. It is obvious from this example, how nearly accurate the supposition, that the chords are divided in the proportion of the times, proves to be for an interval of eight days. I must however observe, that the value of M, and that of the small arcs σ, τ, $A''-A'$, $A'''-A''$, must be computed with great care, in order that the correction deduced from them may not be erroneous.

§. 63.

It is manifest that this is a very simple method of correcting the first computation of the elements, and of obtaining the true result of the three observations in question, not very remote from each other. But the orbit of a comet can never be correctly determined from observations very near each other; partly because all observations must, for many reasons, be inaccurate, and partly for a reason not hitherto much considered, that we cannot depend on the sun's place to single seconds, and before the last labours of Delambre and Von Zach, were liable to still greater errors. An uncertainty or an error of 10″ in the sun's longitude may, in certain circumstances, lead to greater inaccuracy than an error of a minute, or even several minutes, in the observed longitude and latitude of the comet: and this fact ought to serve as a warning to computers, and induce them to determine the place of the sun at each observation with great care. But all errors in the longitude or distance of the sun, or in the observed longitude or latitude of the comet, must naturally have so much the more influence on the elements, as the observations are nearer to each other, and the portion of the orbit in question shorter.

§. 64.

Various methods have been proposed for applying the remotest observations to the correction of the approximate elements of a comet's orbit. They may, however, all be reduced to three; those of Lambert, of Laplace, and of the great Newton. We shall examine them all, and compare them with each other.

§. 65.

Lambert proposes to take the distances of the comet from the earth from the construction, or from a rough calculation, to consider their differences from the truth as differential magnitudes, the higher powers of which may be omitted in the computation; and to determine the amount of these differences from the intervals of time observed. Supposing the approximate

distances from the earth to be a, b, c, he takes for the true distances $a + x$, $b + y$, and $c + z$, expresses with these the distances of the comet from the sun, and all the chords concerned, and compares these, by means of his theorem, with the observed intervals of time: and omitting all the higher powers of x, y, and z, he, of course, obtains their values by linear equations. But this computation is not a little troublesome and tedious; and as I can assert from experience, incomparably more so than might be inferred at first sight from the examples given by LAMBERT.

§. 66.

It is far more convenient to choose two of the approximate elements and to compare them with three observations, in order to see if they agree more or less accurately with them; and then to compute the effect of small alterations in the elements on each observation. Hence the errors of these two elements will become known, and they may be corrected accordingly; and from their corrected value, the other elements of the orbit may be determined or corrected.

§. 67.

LAPLACE chooses for this purpose the time and distance of the perihelium. He then assumes three hypotheses, which, if T be the time of the perihelium, and π the distance, obtained by the approximation, may be thus represented, 1; T, π; 2; $\text{T} + r$, π; 3; T, $\pi + s$; and on each of these hypotheses he computes for the time of three of the remotest observations the differences of the true anomalies, and the distances of the comet from the sun. From the three distances, and the observed geocentric longitudes and latitudes, he finds again, by a calculation not very difficult, the differences of the true anomalies. If these differences agree for one of the hypotheses, the time and distance supposed in it must be correct; if not, we may obtain from these three comparisons the true time and distance, in a manner which will be explained in speaking of the NEWTONIAN method. I do not enter more particularly into the method at

present, because it has been so fully explained by LAPLACE himself in the *Memoirs of the Academy* for 1780, and after him by Pingré in his *Cométographie*.

[Note of the Editor VON ZACH. The work of LAPLACE *On the Motion of the Planets* being rare, the editor thinks it advisable to insert his formulas, in order that all the methods o· correcting the elements of the orbit may be found together and it will give room for remarking the utility of *constant logarithms* which are of use in repeating the computation upon various hypotheses. We find (1) the true anomalies $\varphi', \varphi'', \varphi'''$, from the time and distance of the perihelium, for the respective observations, by means of BARKER's table; as well as the distances from the sun, r', r'', r'''. (2) Then making $\cos \varkappa = \cos \beta \cos (A - \alpha)$ we find

Ist constant L. $= \text{Log } R + \log \sin \varkappa$
IId $= \text{Log} \sin \beta - \log \sin \varkappa$
IIId $= \text{Log } R + \log \sin (A - \alpha)$

Quantities which are obviously independent of the time and distance of the perihelium, and therefore retain the same values notwithstanding their changes. We then make (3)

$\text{Log} \sin K = \text{I c. l.} - \log r$
Angle $\Sigma = K + \varkappa$, or rather $180° - K - \varkappa$
$\text{Log} \sin \lambda = \log \sin \Sigma + \text{II c. l}$
Log sin angle at the comet $= \text{III c. l.} - \log (r \cos \lambda)$
$C = \alpha \pm$ this angle $=$ hel. long. comet.

(4) Reckoning χ', χ'', and χ between the 1st and 2d, 1st and 3d, and 2d and third observations, we have

$\text{Cos } \chi' = \cos (C'' - C') \cos \lambda' \cos \lambda'' + \sin \lambda' \sin \lambda''$
$\text{Cos } \chi'' = \cos (C''' - C') \cos \lambda' \cos \lambda''' + \sin \lambda' \sin \lambda'''$
$\text{Cos } \chi''' = \cos (C''' - C'') \cos \lambda'' \cos \lambda''' + \sin \lambda'' \sin \lambda'''$; two only of these formulas being wanted, and the signs and cosines having been taken out before.

(5) Putting now $\chi' - (\varphi'' - \varphi') = \mu$, and $\chi'' - (\varphi''' - \varphi') = \nu$, we must have, if the time and distance of the perihelium are correct, $\mu = 0$, and $\nu = 0$. But as this will seldom happen, we must alter first the time of the passage of the perihelium only, and then the distance; and by comparing the three values of μ and ν thus

found, we may obtain by interpolation a hypothesis in which both will be correct; and this must again be examined by a new calculation.]

§. 68.

Notwithstanding the convenience and utility of this method' am disposed to believe that the same conciseness and facility may be attained by taking, with NEWTON, for the foundation of the three hypotheses, the longitude of the node, and the inclination of the orbit, instead of the time and distance of the perihelium, and that this method will then possess essential advantages over that of LAPLACE. I call this the NEWTONIAN method, since it could only be by want of recollection that the great EULER, who had certainly read NEWTON's works, and who had no manner of occasion to adorn himself with borrowed plumes, could have mentioned it as his own invention; "*Sequentem methodum sum assecutus.*" *Theor. met. Plan. et Com.* p. 140. It was, however, first suggested by NEWTON, and illustrated in detail by GREGORY, *Princip.* III. 42; although ascribed by many late writers to EULER, without any mention of NEWTON's name.

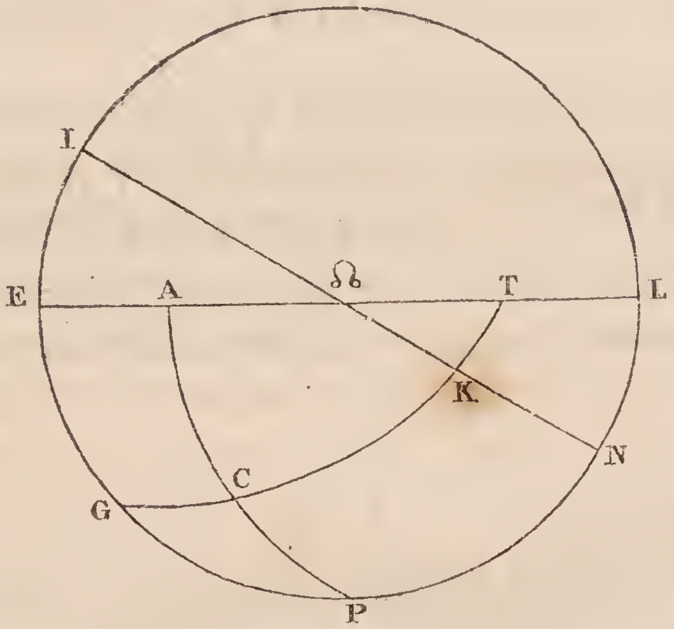
§. 69.

It has commonly been supposed that this method was only to be used with advantage, when an attempt was to be made to find the elements of an elliptic orbit; an experiment which seldom affords any satifactory result; although, if any person chooses to undertake so thankless a labour, he cannot employ a more convenient method. But it may be employed in a much shorter form for the correction of the parabolic elements: as indeed STRUYK has already employed it, although with unnecessary prolixity, and with many superfluous calculations, not being at the time acquainted with the elegant theorem of LAMBERT. *Beschr. der Staartst.* Amst. 1753. I adopted it in a shorter form 17 years ago, in determining the elements of the comet of 1779, from observations which I had made almost without any instruments. *Astr. Jahrb.* Berl. 1782.

§. 70.

In this method we have occasion for the solution of the problem, to determine, from the given position of the orbit with respect to the ecliptic, and the geocentric longitude and latitude of the comet, the heliocentric distance from the node, and the distance of the comet from the sun. Newton takes this solution for granted: Gregory, Euler, and Struyk have entered into the particulars of it; and Lexell, in a separate essay; and Professor Nordmark in an occasional Programma, have endeavoured to simplify and accommodate to practice the formulas subservient to it. And yet it seems to be possible to obtain a more convenient practical solution than has hitherto been made public. It has been usual to employ for the purpose plane trigonometry only; but the problem evidently belongs to spherical trigonometry, since it depends on the relative situation of two planes, the first determined by the centres of the sun, the earth, and the comet; the second, or the orbit of the comet, by the nodes and inclination appropriate to it.

§. 71.



Now let E A ☋ T L be the ecliptic, ☋ the node, here the descending node, I ☋ N the orbit as seen from the sun, T the earth's place, and C the observed geocentric place of the comet. The great circle T K C G being drawn through the place of the comet, its heliocentric place will be K, ☋

K the heliocentric distance of the comet from the node, T K its heliocentric distance from the earth, K C the angle at the comet, and lastly the supplement of T C the geocentric distance of the comet from the sun. [In order that C may represent the geocentric place of the comet, it must be that point of the sphere which is indicated by a line drawn from the sun, supposed to be in its centre, parallel to the direction of the comet from the earth: and this line will make an angle with the revolving radius of the comet, equal to the alternate angle at the comet, which will therefore be represented by K C. Tr.] Now it is obvious that all these distances may be found by the solution of two spherical triangles.

(1) In the right-angled triangle A C T we have T A, the difference of the earth's longitude and the geocentric longitude of the comet, and A C the observed latitude of the comet: whence we have (i.) $\cos TC = \cos TA \cos AC$; and (ii.) $\cot ATC = \cot AC \sin TA$.

(2) In the oblique angled triangle ☊ K T, we have ☊ T the difference of the longitude of the node and of the earth, the angle T ☊ K the inclination of the orbit, and the angle ☊ T K = A T C; hence we find ☊ K and T K by the formulas (iii.) $\tan \frac{1}{2}(☊K + TK) = \dfrac{\cos \frac{1}{2}(☊TK - T☊K)}{\cos \frac{1}{2}(☊TK + T☊K)} \tan \frac{1}{2} ☊T$, (iv.) $\tan \frac{1}{2}(☊K - TK) = \dfrac{\sin \frac{1}{2}(☊TK - T☊K)}{\sin \frac{1}{2}(☊TK + T☊K)} \tan \frac{1}{2} ☊T$. We have then $KC = TC - TK$, and R being the distance of the earth from the sun, and r that of the comet, we have (v.) $r = \dfrac{R \sin TC}{\sin KC}$.

§. 72.

If we compare these formulas with those which have been hitherto employed, we shall be aware of their great convenience especially in the correction of the elements of a comet's orbit. Euler, for example, employs in his *Recherches sur la vraie orbite elliptique de la comète de* 1769, eight proportions, instead of the five here laid down. The whole eight he is obliged to compute for each of the hypotheses, which he assumes for

the longitude of the node and the inclination of the orbit; but here the first, second, and the numerator of the fraction remain the same for all three hypotheses; and besides the coefficient of tang $\frac{1}{2}$ ☊ T is the same for two hypotheses. In short EULER requires 75 logarithms for each observation, this method only 43; while in the computations of LEXELL and NORDMARK about 57 or 60 are employed.

§. 73.

The problem having been reduced to the solution of two cases of spherical triangles, we might easily introduce differential expressions, instead of the three hypotheses, or we might compute the effect of small alterations in the longitude of the node and the inclination of the orbit on the values of ☊ K and r. But I have found by experience that the advantage of this mode of proceeding is not considerable: the quantities themselves may as easily be computed on three hypotheses as the differential formulas, which I am the less disposed to insert here, as they may be investigated with very little trouble.

§. 74.

Having then assumed three hypotheses for the longitude of the node, and for the inclination, we compute upon each of them for the three observations ☊ K $= \xi$, and r: and then the chords between the first and second, and the first and third observations from the formula

$$k' = \sqrt{([r'' - r']^2 + 4\, r'\, r'' \sin \tfrac{1}{2}\, (\xi'' - \xi')^2)}$$
$$k'' = \sqrt{([r''' - r']^2 + 4\, r'\, r''' \sin \tfrac{1}{2}\, (\xi''' - \xi')^2)}$$

We then find from k', k'', and r', r'', r''', the corresponding times between the first and second, and the first and third observations. By comparing these times with those which have been observed, we obtain the true longitude of the node, and the true inclination of the orbit; and hence, by an easy interpolation, the true values of r', r''', ξ', and ξ''', by means of which we find the remaining elements.

§. 75.

In order to have a clear view of the whole proceeding, we may represent the three hypotheses in this manner:

	1 Hyp.	2 Hyp.	3 Hyp.
Longitude of the ☊	☊	☊ $+ p$	☊
Inclination	i	i	$i + q$

p and q amounting to 10, 15, 20, or even more minutes. For each of these hypotheses, and for three observations, we calculate, according to §. 71, ξ', ξ'', ξ''', r', r'', and r'''; and then, by §. 74, k' and k''. We next find the times in each hypothesis, between the first and second, and between the first and third observations, calling them, 1st τ' and τ'', 2ndly $\tau'' + l$, $\tau'' + o$, 3rdly $\tau' + m$, $\tau'' + s$; while the observed intervals are t' and t''. If now the true longitude of the node be ☊ $+ x$, and the true inclination of the orbit $i + y$, we have the equations

$$\frac{xl}{p} + \frac{ym}{q} = t' - \tau',\ \frac{xo}{p} + \frac{ys}{q} = t'' - \tau''; \text{ and hence } x = \frac{(t' - \tau')\,sp - (t'' - \tau'')\,mp}{mo - sl}, \text{ and } y = \frac{(t' - \tau')\,oq - (t'' - \tau'')\,lq}{mo - sl};$$

giving us the true longitude of the node, and the true inclination of the orbit. The true values of r'', r''', ξ', and ξ''', are then found by interpolation; taking for any magnitude which may have been found in the three hypotheses B, B $+ f$, and B $+ g$, the true value B $+ \frac{fx}{p} + \frac{gy}{q}$. It is obvious that, in order to obtain all possible accuracy, we must renew the operation with three new hypotheses for the longitude of the node and the inclination, less remote from each other, if x and y should be found materially greater than p and q, or if large values of p and q had been employed, such as 50, 60, or more minutes. For the method is only so far correct, as the alterations of all the other magnitudes concerned may be considered as proportional to those of the longitude of the node, and of the inclination of the orbit, which is only admissible for small values of p and q; a limitation which is equally applicable to the method of Laplace, and to that which remains to be explained.

[Note of the Editor. The formulas used by LAPLACE considerably resemble these. If the μ and ν, of §. 67, be made for the three hypotheses μ', μ'', μ''', and ν', ν'', ν''', we have

$$y(\mu' - \mu'') + x(\mu' - \mu''') = \mu', \text{ and}$$

$$y(\nu' - \nu'') + x(\nu' - \nu''') = \nu';$$ the resolution and use of these equations being perfectly similar to those of §. 75, and y being the factor by which the alteration of the perihelium distance is multiplied, and x the factor of the alteration of the time of the passage through the perihelium, by means of which the true alterations of these elements are determined. Sometimes, however, it may become necessary to take the second differences into consideration, and the editor has employed LAPLACE's formulas with advantage in the calculations of several comets: he therefore inserts them as applied to LAPLACE's own method; but they may be accommodated, without difficulty, to every other. We are to compute the values of μ and ν for the five following hypotheses; (1) with the elements found by the first approximation; (2) with a small alteration of the perihelium distance; (3) with twice as great an alteration; (4) with the approximate distance unaltered, and with a small alteration of the time; (5) with twice as great an alteration: now if we distinguish the five values of μ and ν by the number of accents, and make x and y the factors of the alterations in the 2d and 4th hypotheses, which are required to find the true alterations of the elements, we may find the values of x and y by the following equations. $0 = (4\mu'' - 3\mu' - \mu''')y + (\mu''' - 2\mu'' + \mu')y^2 + (4\mu'''' - 3\mu' - \mu''''')x + (\mu''''' - 2\mu'''' + \mu')x^2 + 2\mu'$, and $0 = (4\nu'' - 3\nu' - \nu''')y + (\nu''' - 2\nu'' + \nu')y^2 + (4\nu'''' - 3\nu' - \nu''''')x + (\nu''''' - 2\nu'''' + \nu')x^2 + 2\nu'$. We may also observe that we may resolve these equations by extermination, but that we are led by a troublesome operation to an equation of the fourth degree; and that it is therefore more convenient to find approximate values with the omission of the quadratic terms x and y^2, and then to compute these terms with the values so found and to renew the operation, so that x and y may be deduced from the linear equations thus obtained.]

§. 76.

Besides these two methods of correction, I shall explain another which really seems to be the most commodious, where the elements of a parabolic orbit only are in question: and even if it were less advantageous, it would be of use to be in possession of a variety; especially as the two former methods sometimes fail. LAPLACE's method is awkward when the angle at the comet in one of the three observations is very nearly a right one: and NEWTON's computation cannot be employed, when either the inclination of the orbit is very small, or the earth is very near the line of the nodes in one of the observations. Instead of the hypotheses respecting the time and distance of the perihelium, or the situation of the plane of the orbit, we may make three suppositions respecting the curtate distances of the comet from the sun, in two of the remotest observations in our possession; we may compute these curtate distances from the approximate orbit, calling then Δ' and Δ''' and then assume

	1 Hyp.	2 Hyp.	3 Hyp.
1 Obs.	Δ'	$\Delta' + m$	Δ'
3 Obs.	Δ'''	Δ'''	$\Delta''' + n$

We then compute, for Δ' and $\Delta + m$ and the observed position of the comet, the heliocentric longitude and latitude at the time of the first observation, and for Δ''' and $\Delta''' + n$ the heliocentric longitude and latitude in the third observation. These computations are very easy: for the angle at the projected place of the comet, in the plane of the ecliptic, is found by the equation $\sin c = \frac{R \sin (A - \alpha)}{\Delta}$; observing, however, to take the angle acute or obtuse, according to circumstances; and hence we find ε, the elongation of the comet from the earth $= 180° - c - (A - \alpha)$; and for the heliocentric latitude, $\text{tang}\ \lambda = \frac{\text{tang}\ \beta \sin (A - \alpha)}{\sin \varepsilon}$: we then compute, according to §. 42, the longitude of the ascending node, and the inclination of the orbit, for each of the three hypotheses; and since $r' = \Delta' \sec \lambda'$ and $r''' = \Delta''' \sec \lambda'''$, we hence find the true anomalies in both ob-

servations, the distance of the perihelium, and the time from the perihelium to the first and third observations; which gives us the time that ought to elapse between these observations, according to each of the hypotheses: and hence we have the first comparison. We then add, for each of the three orbits, to the time between the perihelium and the first observation, the observed time from the first to a second observation, sufficiently remote from both the others, and compute the respective geocentric longitude; or the latitude, if the latitudes vary more rapidly than the longitudes: and the place so computed gives, together with the observation, the second comparison.

§. 77.

The whole proceeding will therefore stand thus,

		1 Hyp.	2 Hyp.	3 Hyp.	True Orbit.
Curtate dist.	1 Obs.	Δ'	$\Delta'+m$	Δ'	$\Delta'+x$
	3 Obs.	Δ'''	Δ'''	$\Delta'''+n$	$\Delta'''+y$
Time between 1 and 3 obs.		τ	$\tau+p$	$\tau+q$	t'' obs.
Longitude in 2 obs.		α	$\alpha+\nu$	$\alpha+s$	α'' obs.

We have then $\frac{px}{m} + \frac{qy}{n} = t'' - \tau$, and $\frac{rx}{m} + \frac{sy}{n} = \alpha'' - \alpha$; whence we obtain the value of x and y in the same manner as in §. 75. If now m and n have been assumed not too great, and x and y are still smaller, or not considerably larger, the whole of the elements of the orbit may be readily found by interpolation.

§. 78.

It must, however, be remembered, that three complete observations are more than sufficient to determine the orbit of a comet considered as a parabola: that is, if the orbit were not correctly parabolic, or if the observations were erroneous, we could only represent by a parabola three longitudes and two latitudes, or two longitudes and three latitudes: and for this reason I have employed, in the method of correction here explained, only the longitude or the latitude of the middle observation; while in the methods of Lambert and Laplace, as well as in that of Newton here applied to the parabola, we appear to satisfy three complete observations. But this appear-

ance is fallacious; for if there is any want of regularity in the assumed grounds of calculation, some condition of the problem must remain unfulfilled, if we imagine that we have represented the whole of the observations. Thus in LAMBERT's method, §. 65, we shall have determined the intervals of time correctly, but the points found for the places of the comet will not lie in a plane passing through the sun. According to LAPLACE, §. 67, the time and distance of the perihelium, and the true anomalies derived from them, will agree with the observations; but the heliocentric places, derived from them, will again deviate from a great circle. The NEWTONIAN determination of the longitude of the node, and the inclination of the orbit, giving r', r'', r''', and k', k'', correctly, and thence the time agreeing with the observed interval, will indicate places not found in one and the same parabola. In all these cases, therefore, we shall find not one parabola, but strictly speaking three, differing more or less from each other according to the accuracy of the observations, or to the extent of the actual deviation of the comet from a parabola. In LAPLACE's method, the time and distance of the perihelium only are the same for all three; in NEWTON's, the plane of the orbit only is the same; in LAMBERT's, all the five elements belong to three different parabolas. We must adhere in practice to that orbit which agrees perfectly with the first and third observations; passing through the extreme points which have been determined.

§. 79.

The condition that all the points of the orbit must lie in a plane, passing through the sun's centre, is in itself the most essential part of the theory: and hence the NEWTONIAN mode of correction has the advantage, since it satisfies this principal condition: it has also the superior convenience of being immediately applicable to the determination of the elements of an elliptic orbit, if it should happen that a parabola is not sufficient for the observations.

§. 80.

In order to determine if this is the case, we must compute

again ξ'' and r'' from the parabolic elements which have been obtained for the two remotest observations, and compare these values with those which have been already determined according to §. 75. If there is any material difference in the values, supposing that p and q have not been assumed too great, that the observations are accurate, and that they are sufficiently remote, we may then attempt to find an elliptic orbit which shall agree better than the parabolic. I have not been able in this instance to abridge materially the method of EULER, which he has given in the two works already quoted. Instead of the chords k', k'', as soon as we have found ξ', ξ'', ξ''', r', r'', and r''', we must immediately determine the parameter of the ellipse for each of the three hypotheses, by the formula

$$b = \frac{\sin(\xi''-\xi') + \sin(\xi'''-\xi'') - \sin(\xi'''-\xi')}{\frac{\sin(\xi''-\xi')}{r'''} + \frac{\sin(\xi'''-\xi'')}{r'} - \frac{\sin(\xi'''-\xi')}{r''}},$$

which is much more convenient than that which EULER has given in his *Theoria*, but is essentially the same with the calculation contained in his *Recherches*. From the parameter thus found we easily obtain the true anomaly for the first observation, the perihelium distance, the times from the perihelium, and the interval between the observations. For this purpose I prefer the formulas of the *Theoria* to those of the *Recherches*. By comparing the computed with the observed intervals, we determine, as for the parabola, the correction of the longitude of the node and of the inclination, and we thus obtain the true value of the elliptic elements by interpolation.

[Note 6. The elegant theorem here introduced may be thus demonstrated. Calling the distances a, b, and c, the angles formed with the greater axis x, $x+y$, and $x+z$, the parameter p, and the eccentricity e, the greater axis being 1, we obtain, from the well known property of the ellipsis, $a = \frac{p}{1+e\cos x}$, $b = \frac{p}{1+e\cos(x+y)}$, and $c = \frac{p}{1+e\cos(x+z)}$, whence $\cos x = \frac{p-a}{ae}$ $\cos(x+y) = \frac{p-b}{be}$, and $\cos(x+z) = \frac{p-c}{ce}$; but $\cos(x+y) = \cos x \cos y - \sin x \sin y$, whence $\sin y =$

$\frac{\cos x \cos y - \cos(x+y)}{\sin x} = \frac{p-a}{ae \sin x} \cos y - \frac{p-b}{be \sin x}$; and in the same manner $\sin z = \frac{p-a}{ae \sin x} \cos z - \frac{p-c}{ce \sin x}$. Now $\sin (z-y) = \sin z \cos y - \sin y \cos z = \frac{p-a}{ae \sin x} \cos y \cos z - \frac{p-c}{ce \sin x} \cos y - \frac{p-a}{ae \sin x} \cos y \cos z + \frac{p-b}{be \sin x} \cos z = \frac{p-b}{be \sin x} \cos z - \frac{p-c}{ce \sin x} \cos y$; consequently $\frac{p-a}{a} \sin(z-y) = \frac{p-a}{a} \cdot \frac{p-b}{be \sin x} \cos z - \frac{p-a}{a} \cdot \frac{p-c}{ce \sin x} \cos y = \frac{p-b}{b} \sin z - \frac{p-c}{c}$ sin and $\frac{p}{a} \sin(z-y) - \frac{p}{b} \sin z + \frac{p}{c} \sin y = \sin(z-y) - \sin z + \sin y$; whence $p = \frac{\sin y + \sin(z-y) - \sin z}{\frac{\sin y}{c} + \frac{\sin(z-y)}{a} - \frac{\sin z}{b}}$, which is Dr. Olbers's formula. Tr.]

§. 81.

It will seldom or never occur to us to compute the elliptic orbit for the sake of any material utility or advantage. The portion of the orbit, which is in the neighbourhood of the sun, may almost always be so accurately determined by the parabolic hypothesis, that we can represent and predict and judge of its course, and its distance from the earth and the sun, and identify it upon a second appearance with sufficient precision. And this appears to me to be the whole object of the calculation of a comet, since the determination of an elliptic orbit can never ascertain the period of revolution with any certainty, the deviations from the parabolic orbit being always complicated with the errors of observation: and these errors are certainly in many cases greater than could have been imagined, partly from the nature of the light and from the form of the comet, and partly from the imperfections of our catalogues of fixed stars. The comet of 1770, indeed, seems to afford a remarkable and well known exception to the universality of this observation. But without wishing to decide upon this point, we may at least remark, first, that the

observations before the perihelium may have been the more erroneous, as the comet, though without a tail, had still a very considerable apparent diameter, and it is not easy to distinguish the centre of gravity of this mass of vapours as the proper point of observation: and secondly, that the method of NEWTON or of EULER, by which LEXELL determined the ellipsis and the period of this comet, was particularly ill adapted to the occasion, as the orbit was so little inclined to the ecliptic. I do not, however, deny that this paradoxical comet described an ellipsis considerably different from a parabola, since observations so rough as those of LAMBERT, (*Beyträge* III. 318,) are sufficient to show the inadequacy of a parabolic orbit, and even the observations after the perihelium, taken separately, cannot be represented by any one parabola. It is remarkable, that so great a mathematician as DUSE'JOUR should have fancied, tha the had discovered more than one parabola which was capable of giving the observed places at three different times with perfect accuracy. (*Traité* II. xv).

§. 82.

For the determination of elliptic elements the greatest care and accuracy is required in the choice and management of the observations: and we must pay proper regard to parallax, aberration, and nutation. It might, perhaps, be eligible to compute all the observations with the greatest accuracy for a parabola approaching very near to the orbit: the differences of the observations from the computation, so far as they depend on the elliptic form of the orbit, must then exhibit a uniform and regular increase and decrease. Abrupt changes and irregularities imply errors either in the observation or in the computation; and we must not omit to pay attention to single seconds. With these precautions we may be able to form some estimate of the probable errors of the observations, and to make allowance for them; we may then proceed to the investigation of an elliptic orbit, especially when the comet has been seen in both branches of the curve, both before and after its passage through the perihelium.

[The Table of Comets in a future Number.]

Art. XIII.—*Results of some Astronomical Observations made in Blackman-Street, during the Months of March, April, and May,* 1822, *by* James South, F.R.S., *in Lat.* 51° 30′ 3″ N., *Longitude* 21″.76 W.

Eclipses of Jupiter's Satellites.

(Mean Time.)

	h. ′ ″		
March 1, Emersion of 1st Sat. at	6 57 36.60	with	5 feet Equatorial
....	6 57 43.60		30 inch Gregorian.

Diameter of Venus taken in the direction of her Cusps, at about two o'clock, P.M.

March 3, The planet distant from her inferior conjunction } 6 days | mean of | 10 measures = 59″.936

Occultations of Fixed Stars by the Moon's dark Limb.

Star's AR.	Star's Declin.	Mag.	Time of immersion (in Sidereal time).	Instrument employed.
March 1.	° ′		H. ′ ″	
5 44 30 ±	27 41 ± N	6	5 57 4.75	5 feet Equatorial.
5 45 57 ±	27 17 ± N	6	6 53 29.67	Do.
5 47 58 ±	27 32 ± N	6	7 38 35.79	Do.
5 49 10 ±	27 15 ± N	5	8 18 9.71	Do.
5 49 56 ±	27 33 ± N	5	8 39 14.82	Do.
March 2.				
6 47 40 ±	26 19 ± N	6.5	6 21 9.32	Do.
....	....	......	6 21 8.92	9 feet Equatorial.
6 48 22 ±	26 9 ± N	5	6 50 55.83	Do.
....	....	......	6 50 55.73	5 feet Equatorial.
May 24.				
5 57 20 ±	22 6 ±	6.5	13 31 45.98	Achromatic of 3¼ aperture and 45 inches focus.
May 26.				
9 49 9 ±	11 48 ±	7	15 31 20.39	
May 28.				
11 20 16 ±	0 38 ±	8	14 36 52.49	5 feet Equatorial.

No tremulous appearance of either of these Stars, no projection of either of them upon the moon's disk—the disappearance of all instantaneous.

An object glass, 3¼ inches in diameter, and of 45 inches focal length, having been made for me by Tulley, from formulæ given by Mr. Herschel, in a paper recently published in the *Philosophical Transactions*, it became a matter of much interest to ascertain what merit it might possess; and for this purpose it was directed to some of those double stars in which proximity or faintness renders one of them difficult to be seen, and the accompanying diagrams, plate V., are faithful representations of the results: what, therefore, Mr. Herschel's *theory* told him would be good, Mr. Tulley's *practice* has declared so. The magnifying power employed upon the stars was about 300—upon the Nebula about 50.

At the request of several astronomical friends, the corrections in right ascension of the thirty-six principal stars are here resumed; and tables for converting space into time and the contrary, are subjoined for the convenience of the practical astronomer.

Art. XIV.—The mean places of 46 Greenwich Stars, Reduced to Jan. 1, 1822.

Names of Stars.	Right Ascension.	N. P. D.	Declination.
γ Pegasi	0 4 5.00	75 48 19.75	14 11 40.25 N
α Cassiopeiæ .	0 30 27.61	34 26 24.48	55 33 35.52 N
Polaris . . .	0 57 30.03	1 38 26.67	88 21 33.33 N
α Arietis	1 57 9.71	67 22 59.89	22 37 0.11 N
α Ceti	2 52 59.13	86 36 48.69	3 23 11.31 N
α Persei	3 11 39.65	40 46 50.87	49 13 9.13 N
Aldebaran . .	4 25 43.14	73 51 23.77	16 8 36.23 N
Capella . . .	5 3 33.41	44 11 39.35	45 48 20.65 N
Rigel	5 5 59.32	98 24 50.01	8 24 50.01 S
β Tauri	5 15 2.97	61 33 9.23	28 26 50.77 N
α Orionis . . .	5 45 32.42	82 38 3.30	7 21 56.70 N
Sirius	6 37 18.12	106 28 39.39	16 28 39.39 S
Castor	7 23 13.81	57 43 50.43	32 16 9.57 N
Procyon . . .	7 29 58.97	84 19 32.77	5 40 27.23 N
Pollux	7 34 24.85	61 33 8.45	28 26 51.55 N
α Hydræ . . .	9 18 50.51	97 53 27.57	7 53 27.57 S
Regulus . . .	9 58 53.16	77 9 58.28	12 50 1.72 N
α Ursæ Maj. . .	10 52 39.65	27 17 25.20	62 42 34.80 N
β Leonis	11 39 58.59	74 25 58.01	15 34 1.99 N
β Virginis . . .	11 41 25.59	87 13 54.66	2 46 5.34 N
γ Ursæ Maj. . .	11 44 25 53	35 18 55.25	54 41 4.75 N
Polaris, S. P.	12 57 30.03	1 38 26.67	88 21 33.33 N
Spica Virg. .	13 15 49.74	100 13 41.18	10 13 41.18 S
η Ursæ Maj. . .	13 40 31.19	39 47 41.61	50 12 18.39 N
Arcturus . .	14 7 32.90	69 53 9.87	20 6 50.13 N
1 α Libræ . . .	14 40 51.61	105 14 56.00	15 14 56.00 S
2 α Libræ . . .	14 41 3.05	105 17 40.13	15 17 40.13 S
β Ursæ Minor .	14 51 19.72	15 7 1.23	74 52 58.77 N
α Coronæ Bor. .	15 27 9.40	62 40 47.76	27 19 12.24 N
α Serpentis . .	15 35 30.55	83 0 24.79	6 59 35.21 N
Antares . . .	16 18 30.55	116 1 33.34	26 1 33.34 S
α Herculis . . .	17 6 32.29	75 23 54.55	14 36 5.45 N
α Ophiuchi . .	17 26 40.70	77 18 6.75	12 41 53.25 N
γ Draconis . .	17 52 28.65	38 29 9.85	51 30 50.15 N
α Lyræ	18 30 54.95	51 22 33.45	38 37 26.55 N
γ Aquilæ . .	19 37 47.98	79 48 45.46	10 11 14.54 N
α Aquilæ . .	19 42 6.00	81 35 37.27	8 24 22.73 N
β Aquilæ . .	19 46 34.31	84 1 46.99	5 58 13.01 N
1 α Capricorni	20 7 46.56	103 2 59.00	13 2 59.00 S
2 α Capricorni	20 8 10.31	103 5 15.75	13 5 15.75 S
α Cygni	20 35 22.14	45 21 3.33	44 38 56.67 N
α Cephei	21 14 19.55	28 9 58.45	61 50 1.55 N
β Cephei	21 26 19.58	20 13 9.24	69 46 50.76 N
α Aquarii . . .	21 56 38.44	91 10 45.81	1 10 45.81 S
Fomalhaut . .	22 47 47.64	120 33 47.60	30 33 47.60 S
α Pegasi . . .	22 55 54.20	75 44 57.10	14 15 2.90 N
α Andromedæ .	23 59 12.52	61 53 30.33	28 6 29.67 N

JULY	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Vir.	Arcturus.
1	+2.35″	+1.82″	+1.55″	+1.33″	+1.41″	+0.93″	+1.34″	+1.11″	+0.57″	+1.43″	+1.07″	+1.40″	+1.14″	+1.64″	+2.09″	+2.04″	+2.50″	+2.76″
2	39	85	58	35	44	95	37	13	59	44	08	40	13	64	08	04	49	75
3	42	88	61	38	47	97	39	15	60	45	09	41	13	63	07	03	48	74
4	45	91	63	40	50	99	41	16	61	45	09	42	13	63	07	02	47	73
5	48	95	66	42	53	+1.01	43	18	62	46	10	43	13	63	06	01	46	71
6	51	98	69	45	56	03	45	20	63	47	11	44	13	62	05	00	45	70
7	55	+2.01	72	47	59	05	48	22	65	48	12	44	12	62	04	00	44	69
8	58	05	75	50	62	07	50	23	66	49	12	45	12	61	03	+1.99	43	68
9	61	08	78	52	65	09	52	25	67	50	13	46	12	61	02	98	42	67
10	64	11	81	55	68	11	55	27	69	51	14	47	12	61	01	97	41	66
11	67	15	84	58	71	13	57	29	71	53	15	48	12	60	00	96	41	65
12	70	18	87	61	74	16	60	31	73	54	17	50	12	60	00	96	39	64
13	73	22	90	64	78	19	64	34	75	57	19	52	13	60	00	96	39	63
14	76	26	93	67	81	21	67	36	76	58	20	53	13	60	00	95	38	62
15	79	30	96	70	84	23	70	38	77	60	21	54	13	60	+1.99	95	37	61
16	82	33	99	73	87	25	72	40	79	61	22	55	13	60	98	94	36	60
17	85	36	+2.02	75	90	28	75	42	80	63	24	57	13	60	97	94	35	58
18	88	40	05	78	94	30	77	44	82	64	25	58	13	59	96	93	34	57
19	91	43	08	81	97	32	80	46	83	66	26	59	13	59	95	92	33	56
20	94	46	11	84	+2.00	34	83	48	85	68	27	61	13	59	94	91	32	55
21	97	49	14	87	04	37	86	51	86	69	28	62	14	59	93	91	31	53
22	99	53	17	90	07	39	88	53	88	71	30	64	14	59	93	90	30	52
23	+3.02	56	21	93	11	42	91	55	90	73	31	65	14	59	92	89	29	50
24	05	59	24	95	14	44	94	57	91	74	32	67	14	59	91	89	28	49
25	08	62	27	98	18	46	97	60	93	76	34	69	15	60	90	88	26	48
26	11	65	30	+2.01	21	49	+2.00	62	95	78	35	70	15	60	89	87	25	46
27	13	69	34	04	25	51	02	64	97	80	36	72	15	60	89	86	24	45
28	16	72	37	07	28	54	05	67	98	81	38	73	16	60	88	86	23	43
29	19	76	40	10	32	56	08	69	+1.00	83	39	75	17	60	87	85	22	42
30	22	80	43	13	36	59	11	71	02	85	41	77	18	60	86	85	21	41
31	25	84	46	16	40	62	15	74	04	87	43	79	18	61	86	84	20	39

JULY continued	α Libræ.	α Cor.Bor.	α Serpent.	Antares.	α Herculis	α Ophiuch	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	α Capric.	α Cygni.	α Aquarii.	Fomalh.	α Pegasi.	α Androm.	Polaris.
	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	H. M. S.
1	+3.07	+3.06	+3.17	+3.95	+3.33	+3.38	+3.16	+3.38	+3.43	+3.44	+3.79	+2.86	+3.14	+3.44	+2.68	+2.27	0 . 57 . 22.50
2	07	06	17	95	33	38	16	39	44	45	81	88	16	47	71	30	23.32
3	06	05	16	95	33	38	17	40	46	47	83	89	19	51	74	34	24.10
4	05	04	16	95	33	38	17	41	47	48	84	91	21	54	76	37	24.84
5	04	03	15	94	33	39	18	43	48	50	86	93	24	57	79	40	25.54
6	03	02	15	94	33	39	18	44	50	51	88	95	26	60	82	44	26.22
7	03	02	14	94	33	39	18	45	51	53	90	97	29	63	85	47	26.90
8	02	01	14	93	33	39	19	47	53	54	91	99	31	67	88	51	27.59
9	01	00	13	93	33	40	19	48	54	56	93	+3.01	34	70	91	54	28.31
10	00	+2.99	12	93	33	40	19	49	55	57	94	02	36	73	94	57	29.06
11	+2.99	98	12	92	34	40	19	50	56	58	96	04	38	77	96	60	29.86
12	99	97	11	92	34	40	19	51	57	59	98	06	41	80	99	63	30.69
13	98	96	11	92	34	41	20	53	59	61	+4.00	08	44	84	+3.02	67	31.51
14	98	95	10	92	34	41	20	55	60	62	01	09	47	87	05	71	32.37
15	97	94	10	91	33	41	20	56	62	64	02	10	49	91	08	75	33.18
16	96	93	09	91	33	41	20	57	63	65	04	12	51	94	10	78	33.96
17	95	91	09	90	33	40	20	58	64	66	05	13	53	97	13	81	34.69
18	94	90	08	90	32	40	20	59	65	67	07	15	56	+4.00	15	84	35.39
19	93	89	07	89	32	40	20	60	66	68	08	16	58	03	18	87	36.05
20	92	88	06	88	31	40	19	60	67	69	09	17	60	06	20	90	36.69
21	91	86	05	87	30	39	19	61	67	69	+4.10	18	62	08	22	93	37.34
22	90	85	04	86	30	39	18	61	68	70	11	18	63	10	25	95	38.00
23	89	83	03	85	29	38	18	62	68	71	12	19	65	12	27	98	38.70
24	88	82	02	85	28	38	17	62	69	71	13	20	67	14	29	+3.01	39.43
25	86	80	00	84	27	37	16	63	70	72	14	21	69	16	31	04	40.19
26	85	79	+2.99	83	26	37	16	63	70	72	15	22	71	18	33	07	40.98
27	84	77	98	82	26	36	15	64	71	73	16	22	72	20	36	09	41.78
28	83	76	97	81	25	36	15	64	71	73	17	23	74	22	38	12	42.57
29	82	74	96	80	24	35	14	65	72	73	18	24	76	24	40	15	43.34
30	81	73	95	79	23	34	13	65	72	74	19	24	77	26	42	18	44.06
31	80	71	94	78	22	33	12	65	72	74	20	25	79	28	44	21	44.72

AUG.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virgin.	Spic. Vir.	Arcturus.
	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″
1	+3.28	+2.87	+2.50	+2.20	+2.45	+1.65	+2.18	+1.78	+1.07	+1 90	+1.45	+1.82	+1.20	+1.61	+1.86	+1.84	+2.20	+2.38
2	31	90	53	23	49	68	22	81	09	93	47	84	21	62	85	84	20	37
3	33	93	56	26	53	70	25	83	11	95	48	86	21	62	85	84	19	36
4	36	96	59	29	57	73	28	86	14	98	50	88	22	63	84	83	17	35
5	38	99	62	32	61	76	31	88	16	+2.00	52	90	23	63	83	83	16	33
6	41	+3.03	65	35	64	79	34	91	18	02	54	92	24	63	82	82	15	32
7	43	06	68	38	68	81	37	93	20	04	55	94	25	64	82	82	14	30
8	46	09	71	41	72	84	40	96	22	06	57	96	25	64	81	81	13	29
9	48	12	74	44	76	87	43	99	24	09	59	98	26	64	81	81	12	28
10	50	15	77	47	80	89	46	+2.01	26	11	61	+2.01	27	65	80	80	11	26
11	52	18	80	50	84	92	50	04	29	13	63	03	28	65	80	80	10	25
12	54	21	83	53	88	95	53	07	31	16	65	05	29	66	80	79	09	23
13	56	24	85	56	92	97	56	09	33	18	67	07	29	66	80	79	08	22
14	59	27	88	60	97	+2.00	59	12	35	20	69	09	30	67	79	79	07	20
15	61	30	91	63	+3.01	03	62	15	37	23	71	11	31	67	79	78	06	19
16	63	33	94	66	05	05	66	18	40	25	73	14	32	68	79	78	05	17
17	65	36	97	69	09	08	69	20	42	28	75	16	33	68	78	77	04	16
18	67	39	+3.00	72	13	11	72	23	44	30	77	18	34	69	78	77	03	14
19	69	42	03	75	17	14	75	26	47	33	79	20	35	70	78	76	02	13
20	71	45	06	80	22	17	79	29	50	36	82	23	37	72	79	76	02	12
21	73	48	09	83	27	20	83	32	52	39	84	26	39	73	79	75	01	11
22	75	51	12	86	31	23	86	35	54	41	86	29	40	74	79	74	00	09
23	77	54	15	89	35	26	89	37	56	44	88	31	41	75	79	73	00	08
24	79	57	18	92	40	29	93	40	59	47	91	33	42	76	78	71	+1.99	07
25	81	60	21	95	44	32	96	43	61	49	93	36	43	77	78	70	98	05
26	83	62	23	98	48	34	99	46	63	52	95	38	45	78	78	69	97	04
27	85	65	26	+3.01	52	37	+3.03	48	66	54	97	41	46	79	78	68	96	02
28	87	68	29	04	56	40	06	51	68	57	99	43	47	80	78	67	95	01
29	88	71	32	07	60	43	09	54	71	60	+2.01	46	48	81	78	68	93	00
30	90	73	34	10	65	46	13	57	74	63	04	48	50	82	78	69	92	+1.99
31	91	75	37	13	69	49	16	60	76	66	06	51	51	84	78	71	90	97

AUG. continued	α Libræ.	α Cor.Bor.	α Serpent.	Antares.	α Herculis	α Ophiuch	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	α Capric.	α Cygni.	α Aquarii.	Fomalh.	α Pegasi.	α Androm.	Polaris.
	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	H. M. S.
1	+2.79	+2.71	+2.93	+3.78	+3.21	+3 33	+3.12	+3.66	+3.74	+3.75	+4.21	+3.25	+3.81	+4.31	+3.47	+3.24	0 . 57 . 45.34
2	78	69	92	77	21	32	11	66	74	75	22	26	83	33	50	27	45.93
3	77	68	91	76	20	31	10	67	74	76	22	27	84	35	52	30	46.53
4	76	66	90	75	19	30	09	67	74	76	23	27	86	38	54	33	47.12
5	75	65	89	74	18	29	08	67	74	77	23	28	87	40	56	36	47.72
6	73	63	87	72	17	28	07	68	75	77	24	28	89	42	58	39	48.35
7	72	62	86	71	16	27	06	68	75	77	24	29	90	44	60	41	49.02
8	71	60	85	70	15	26	05	68	75	78	25	29	92	46	62	44	49.74
9	70	58	84	69	14	25	04	68	75	78	25	29	93	48	64	46	50.45
10	68	56	82	67	12	24	02	67	74	78	25	28	94	49	65	48	51.17
11	67	55	81	66	11	22	01	67	74	77	25	28	95	51	67	51	51.86
12	65	53	79	64	09	21	+2.99	67	74	77	25	28	96	53	68	53	52.53
13	64	51	78	63	08	20	98	67	74	77	25	28	97	54	70	55	53.17
14	63	49	77	62	07	19	97	66	72	77	26	27	98	56	72	57	53.73
15	61	47	75	60	05	18	95	66	72	77	26	27	99	58	73	59	54.25
16	60	46	74	59	04	16	94	66	72	76	26	27	+4.00	60	75	62	54.76
17	58	44	72	57	02	15	92	65	71	76	26	26	01	61	76	64	55.26
18	57	42	71	56	01	14	91	65	71	76	26	26	02	63	78	66	55.77
19	56	40	70	55	00	13	89	65	71	76	26	25	03	64	79	68	56.29
20	56	39	69	55	+2.99	12	88	64	70	76	26	25	04	66	81	71	56.83
21	55	37	68	54	98	11	87	64	70	75	26	24	05	68	82	73	57.42
22	54	35	66	52	96	09	85	63	70	75	25	24	05	69	83	75	58.04
23	53	34	65	51	95	08	83	63	69	74	25	23	06	70	84	76	58.68
24	51	32	63	49	93	06	81	62	69	73	25	22	07	71	86	78	59.28
25	50	30	62	48	92	05	79	62	69	73	24	21	07	72	87	80	59.88
26	49	28	60	46	90	03	78	61	68	72	24	20	08	74	88	82	0 . 58 . 0.43
27	47	26	59	45	89	02	76	61	68	72	23	19	08	75	89	84	0.94
28	46	24	57	43	87	00	74	60	67	71	23	18	09	76	90	86	1.40
29	45	22	56	41	85	+2.98	72	59	67	70	22	17	09	77	91	87	1.80
30	44	20	54	40	84	97	70	58	65	69	22	16	09	78	92	89	2.18
31	42	19	53	38	82	95	69	56	64	68	21	15	10	78	92	90	2.57

SEPT.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis	Spica Vir.	Arcturus.
	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″
1	+3.93	+3.78	+3.40	+3.16	+3.73	+2.52	+3.20	+2.63	+1.79	+2.69	+2.09	+2.54	+1.53	+1.85	+1.78	+1.72	+1.88	+1.96
2	94	80	42	19	77	54	23	65	82	72	11	56	54	86	78	73	87	95
3	95	83	45	23	82	57	26	68	85	75	13	59	56	87	79	74	85	94
4	97	85	48	26	86	60	30	71	88	78	16	62	57	88	79	75	83	93
5	98	88	51	29	90	63	33	74	91	81	19	65	59	90	79	77	81	92
6	+4.00	90	53	32	95	66	37	77	94	84	22	67	60	91	79	78	80	91
7	01	93	56	35	+4.00	69	41	80	97	88	25	70	62	92	80	79	78	90
8	03	96	59	39	05	73	45	84	+2.00	92	28	74	65	94	80	80	80	89
9	05	99	61	43	10	76	49	87	02	95	30	77	66	96	81	81	81	88
10	06	+4.02	63	46	15	79	52	90	05	98	33	80	68	98	81	82	81	87
11	07	04	65	49	19	81	56	93	08	+3.01	35	83	70	+2.00	82	82	82	86
12	08	06	67	52	23	84	59	95	10	04	38	86	71	01	82	82	82	85
13	09	08	70	55	27	87	62	98	13	08	41	89	73	03	83	82	83	83
14	10	10	72	58	31	90	66	+3.01	16	11	43	92	75	04	83	83	83	82
15	11	13	74	61	36	93	69	04	19	14	46	95	77	06	84	83	83	81
16	12	15	76	64	40	96	73	07	21	17	48	98	78	07	84	84	83	80
17	13	17	78	67	44	99	76	10	24	20	51	+3.01	80	09	85	84	83	79
18	14	19	80	70	48	+3.02	79	13	27	23	54	04	82	11	86	85	84	78
19	14	21	83	73	52	05	83	16	30	27	56	07	84	13	86	86	84	77
20	15	23	85	76	56	07	86	19	32	30	59	10	86	14	87	86	84	77
21	16	25	88	79	60	10	89	22	35	33	62	13	88	16	88	87	84	76
22	16	27	90	81	64	13	92	25	38	36	64	16	90	18	88	88	84	75
23	17	30	93	84	69	16	96	28	41	40	67	20	92	20	89	89	84	74
24	18	32	95	87	73	19	99	31	44	43	70	23	94	22	90	90	84	73
25	19	34	98	90	77	21	+4.02	34	46	46	73	26	97	23	91	90	84	73
26	20	36	+4.01	93	81	24	06	37	49	50	76	29	+2.00	25	92	91	85	72
27	21	39	04	97	86	27	10	42	+2.53	54	79	33	03	28	93	93	85	72
28	21	41	07	+4.00	91	30	14	45	56	57	82	37	05	30	94	94	85	72
29	22	43	09	02	95	33	17	48	59	60	85	41	07	32	95	95	85	71
30	22	44	11	05	99	36	21	51	62	64	87	44	10	34	97	96	85	71

SEPT. continued	α Libræ.	α Cor.Bor	α Serpent.	Antares.	α Herculis	α Ophiuch	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	α Capric.	α Cygni.	α Aquarii	Fomalh.	α Pegasi.	α Androm	Polaris.
	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	H. M. S.
1	+2.41	+2.17	+2.51	+3.36	+2.80	+2.94	+2.65	+3.55	+3 63	+3.67	+4.20	+3.14	+4.10	+4.79	+3.93	+3.92	0 . 58 . 2.97
2	40	15	50	35	79	92	63	54	62	67	20	12	10	80	94	93	3.40
3	39	13	48	33	77	90	61	53	61	66	19	11	10	81	95	95	3.84
4	38	11	47	31	75	89	59	51	60	65	18	10	10	82	96	96	4.32
5	37	10	45	29	73	87	56	50	59	64	17	09	11	82	96	98	4.82
6	36	08	44	28	72	86	54	49	58	63	17	07	11	83	97	99	5.33
7	35	06	42	26	70	84	52	48	58	62	16	06	11	84	98	+4.01	5.84
8	34	04	42	26	69	83	50	48	57	62	16	05	13	85	99	03	6.28
9	33	02	40	25	67	81	47	47	56	61	15	04	13	86	+4.00	05	6.69
10	32	01	39	23	66	80	45	46	55	60	14	02	12	86	00	06	7.06
11	31	+1.99	37	22	64	78	43	45	54	59	13	00	12	86	01	07	7.38
12	30	97	36	20	62	76	41	44	53	58	12	+2.99	12	86	01	08	7.67
13	29	95	34	18	60	74	38	42	51	56	11	97	12	87	02	09	7.92
14	28	93	33	17	58	72	36	41	50	55	10	95	12	87	02	10	8.17
15	27	92	31	15	57	71	34	40	49	54	09	93	11	87	02	11	8.45
16	26	90	30	14	55	69	31	38	47	52	08	92	11	88	03	12	8.75
17	25	88	28	12	53	67	29	37	46	51	07	90	11	88	03	13	9.10
18	24	86	27	10	51	65	27	35	45	50	06	88	11	88	03	14	9.44
19	23	85	25	09	49	63	24	34	43	48	04	86	10	88	03	14	9.80
20	22	83	24	07	48	62	22	32	42	47	03	84	10	87	03	15	10.18
21	21	82	23	06	46	60	19	31	40	45	02	82	09	87	03	15	10.53
22	21	80	22	04	44	58	17	29	39	44	01	80	09	87	03	16	10.86
23	20	78	20	02	42	56	14	27	37	42	+3.99	78	08	87	02	17	11.11
24	19	77	19	01	40	54	12	26	36	41	98	75	08	87	02	17	11.32
25	18	75	18	+2.99	39	53	10	24	34	39	97	73	07	87	02	18	11.47
26	17	74	16	98	37	51	08	23	33	38	95	71	07	87	02	18	11.60
27	17	73	16	98	36	50	05	22	32	37	95	70	07	87	02	19	11.73
28	16	72	15	97	34	48	03	20	30	35	94	68	06	86	02	19	11.84
29	16	70	14	95	33	47	00	19	29	34	93	66	05	86	02	20	11.96
30	15	69	13	94	31	45	+1.98	17	27	32	92	64	04	86	02	20	12.12

ART. XVI.—i. *Table of Equivalents, for converting Hours, Minutes, and Seconds, into Space.*

For Hours.		For Minutes.				For Seconds.				For Decimal Parts.	
H.	Deg.	Min.	Deg. M.	Min.	Deg. M.	Sec.	Min. Sec.	Sec.	Min. Sec.	Sec.	Sec.
1	15	1	0 15	31	7 45	1	0 15	31	7 45	.1	1.5
2	30	2	0 30	32	8 0	2	0 30	32	8 0	.2	3.0
3	45	3	0 45	33	8 15	3	0 45	33	8 15	.3	4.5
4	60	4	1 0	34	8 30	4	1 0	34	8 30	.4	6.0
5	75	5	1 15	35	8 45	5	1 15	35	8 45	.5	7.5
6	90	6	1 30	36	9 0	6	1 30	36	9 0	.6	9.0
7	105	7	1 45	37	9 15	7	1 45	37	9 15	.7	10.5
8	120	8	2 0	38	9 30	8	2 0	38	9 30	.8	12.0
9	135	9	2 15	39	9 45	9	2 15	39	9 45	.9	13.5
10	150	10	2 30	40	10 0	10	2 30	40	10 0		
11	165	11	2 45	41	10 15	11	2 45	41	10 15	.01	.15
12	180	12	3 0	42	10 30	12	3 0	42	10 30	.02	.30
13	195	13	3 15	43	10 45	13	3 15	43	10 45	.03	.45
14	210	14	3 30	44	11 0	14	3 30	44	11 0	.04	.60
15	225	15	3 45	45	11 15	15	3 45	45	11 15	.05	.75
16	240	16	4 0	46	11 30	16	4 0	46	11 30	.06	.90
17	255	17	4 15	47	11 45	17	4 15	47	11 45	.07	1.05
18	270	18	4 30	48	12 0	18	4 30	48	12 0	.08	1.20
19	285	19	4 45	49	12 15	19	4 45	49	12 15	.09	1.35
20	300	20	5 0	50	12 30	20	5 0	50	12 30		
21	315	21	5 15	51	12 45	21	5 15	51	12 45	.001	.015
22	330	22	5 30	52	13 0	22	5 30	52	13 0	.002	.030
23	345	23	5 45	53	13 15	23	5 45	53	13 15	.003	.045
24	360	24	6 0	54	13 30	24	6 0	54	13 30	.004	.060
		25	6 15	55	13 45	25	6 15	55	13 45	.005	.075
		26	6 30	56	14 0	26	6 30	56	14 0	.006	.090
		27	6 45	57	14 15	27	6 45	57	14 15	.007	.105
		28	7 0	58	14 30	28	7 0	58	14 30	.008	.120
		29	7 15	59	14 45	29	7 15	59	14 45	.009	.135
		30	7 30	60	15 0	30	7 30	60	15 0		

ii. *Table of Equivalents, for converting Degrees, Minutes, and Seconds, into Time.*

For Degrees.						For Minutes.						For Seconds.				For Decimal Parts.	
Deg.	H.	M.	Deg.	H.	M.	Min.	′	″	Min.	′	″	Sec.	″	Sec.	″	Sec.	″
1	0	4	31	2	4	1	0	4	31	2	4	1	0.0667	31	2.0667	.1	.0067
2	0	8	32	2	8	2	0	8	32	2	8	2	0.1333	32	2.1333	.2	.0133
3	0	12	33	2	12	3	0	12	33	2	12	3	0.2000	33	2.2000	.3	.0200
4	0	16	34	2	16	4	0	16	34	2	16	4	0.2667	34	2.2667	.4	.0267
5	0	20	35	2	20	5	0	20	35	2	20	5	0.3333	35	2.3333	.5	.0333
6	0	24	36	2	24	6	0	24	36	2	24	6	0.4000	36	2.4000	.6	.0400
7	0	28	37	2	28	7	0	28	37	2	28	7	0.4667	37	2.4667	.7	.0467
8	0	32	38	2	32	8	0	32	38	2	32	8	0.5333	38	2.5333	.8	.0533
9	0	36	39	2	36	9	0	36	39	2	36	9	0.6000	39	2.6000	.9	.0600
10	0	40	40	2	40	10	0	40	40	2	40	10	0.6667	40	2.6667		
11	0	44	41	2	44	11	0	44	41	2	44	11	0.7333	41	2.7333	.01	.0007
12	0	48	42	2	48	12	0	48	42	2	48	12	0.8000	42	2.8000	.02	.0013
13	0	52	43	2	52	13	0	52	43	2	52	13	0.8667	43	2.8667	.03	.0020
14	0	56	44	2	56	14	0	56	44	2	56	14	0.9333	44	2.9333	.04	.0027
15	1	0	45	3	0	15	1	0	45	3	0	15	1.0000	45	3.0000	.05	.0033
16	1	4	46	3	4	16	1	4	46	3	4	16	1.0667	46	3.0667	.06	.0040
17	1	8	47	3	8	17	1	8	47	3	8	17	1.1333	47	3.1333	.07	.0047
18	1	12	48	3	12	18	1	12	48	3	12	18	1.2000	48	3.2000	.08	.0053
19	1	16	49	3	16	19	1	16	49	3	16	19	1.2667	49	3.2667	.09	.0060
20	1	20	50	3	20	20	1	20	50	3	20	20	1.3333	50	3.3333		
21	1	24	100	6	40	21	1	24	51	3	24	21	1.4000	51	3.4000	.001	.0001
22	1	28	150	10	0	22	1	28	52	3	28	22	1.4667	52	3.4667	.002	.0001
23	1	32	200	13	20	23	1	32	53	3	32	23	1.5333	53	3.5333	.003	.0002
24	1	36	250	16	40	24	1	36	54	3	36	24	1.6000	54	3.6000	.004	.0003
25	1	40	300	20	0	25	1	40	55	3	40	25	1.6667	55	3.6667	.005	.0003
26	1	44	350	23	20	26	1	44	56	3	44	26	1.7333	56	3.7333	.006	.0004
27	1	48	360	24	0	27	1	48	57	3	48	27	1.8000	57	3.8000	.007	.0005
28	1	52				28	1	52	58	3	52	28	1.8667	58	3.8667	.008	.0005
29	1	56				29	1	56	59	3	56	29	1.9333	59	3.9333	.009	.0006
30	2	0				30	2	0	60	4	0	30	2.0000	60	4.0000		

Art. XVII. PROGRESS OF FOREIGN SCIENCE

I. Chemical Science.

i. *Analysis of the Mineral and Thermal Waters of Saint Nectaire, Department of the Puy-de-Dome. By* M. P. Berthier, *Engineer of Mines.*

	Anhydrous Salts.	Crystalline Salts.
Free carbonic acid	0.000736	0.000736
Bi-carbonate of soda	0.002833	0.003150
Muriate of soda	0.002420	0.002420
Sulphate of soda	0.000156	0.000350
Carbonate of lime	0.000440	0.000440
Carbonate of magnesia	0.000240	0.000240
Silica	0.000100	0.000100
Oxide of iron	0.000014	0.000014
	0.006203	0.006714

Or, supposing the soda in the state of subcarbonate—

	Anhydrous Salts.	Crystalline Salts.
Free carbonic acid	0.001545	0.001545
Subcarbonate of soda	0.002024	0.005419
Muriate of soda	0.002420	0.002420
Sulphate of soda	0.000156	0.000350
Carbonate of lime, &c.	0.000794	0.000794
	0.005394	0.008983

There are, therefore, few mineral waters so rich in alkaline salts, as those of Saint Nectaire. They are, however, much inferior in this respect to the waters of Vichy, in Auvergne, which contain 0.0038 of anhydrous carbonate of soda, whilst there is only 0.002 in those of Saint Nectaire.—*Ann. de Ch. et de Phys.*, xix. 122.

On the Subnitrates, and Super-nitrates. By M. P. Grouvelle.—Berzelius has given the analysis of the subnitrates of lead and copper, in his *Essay on the Theory of Chemical Proportions.* It thence appears that in the nitrates which he examined, the oxygen of the nitric acid is to the oxygen of the oxide, as 5 to 1, 2, 3, 6; but he does not seem to have studied any other subnitrates. 2.347 gr. of a subnitrate of zinc were found to consist of—

Oxide	1.948	4 atoms	81.69
Acid	0.328	1 ——	13.75
Water	0.109	2 ——	4.56
	2.385		100.00

Subnitrate of iron, prepared by a powerful evaporation, and dried in a glass tube, till it was on the verge of decomposition, consisted of—

Tritoxide of iron	1.543	4	atoms	81.26
Nitric acid	0.267	1	——	14.06
Water	0.088	2	——	4.68
	1.198			100.00

Subnitrate of bismuth. 2.827 parts of the salt, precipitated by water, and dried with precaution on the fire, were found to consist of—

Oxide	2	atoms	2.405
Acid	1	——	0.413
			2.818

5.348 of the same subnitrate, dried *in vacuo*, by means of sulphuric acid, left 4.353 of oxide after calcination; they consisted of—

Oxide	4.353	2	atoms	81.37
Acid	0.747	1	——	13.97
Water	0.248	2	——	4.66
				100.00

Sub-protonitrate of mercury, precipitated by water. Its black oxide was separated by potash in excess, collected on a filter and weighed. 8.979 parts yielded 7.947 of oxide.

Oxide of mercury	7.947	2	atoms	88.60
Acid	1.022	1	——	11.40

Sub-deutonitrate of mercury, precipitated by water, gave a compound of—

Oxide	3.901	2	atoms	88.97
Acid	0.484	1	——	11.03
	4.385			100.00

When it is precipitated, by alkali not in excess, this sub-deutonitrate cannot be washed, because water decomposes it instantly and completely; and even the preceding sub-deutonitrate is destroyed by boiling water, which carries off the whole of the acid, with a little oxide. It becomes, therefore, necessary to filter the sub-deutonitrate separated by the potash, to compress it between papers, and dry it *in vacuo*. It was then of a fine yellow colour, and did not decompose in the air. Two analyses gave more acid than was requisite for the constitution of two atoms of oxide and one of acid. But as it still retained much neutral nitrate, it is almost certain, says M. Grouvelle, that its composition was as follows:—

2 atoms oxide	. . .	5.910,	quantity obtained.
1 —— acid	. . .	0.703	
		0.613	

6.8 parts had been employed.

"It follows," says he, "from these analyses, that in the nitrates hitherto examined, the oxygen of the acid is to that of the oxide, as 5 to 1, 2, 3, 4, 6, 7, 8."

Of the acidulous nitrates. As we now know the composition of the subnitrates of bismuth and mercury, in order to learn what takes place when the neutral nitrates are decomposed by water, we must examine the proportion between the quantity of oxide precipitated in the state of subnitrate, and that which remains in solution.

10.142 gr. of crystallized nitrate of bismuth, dried at the fire, to expel as much as possible the excess of acid, were treated with a great quantity of water. There was thus obtained—

5.493 gr. of subnitrate, retaining still a little humidity besides the water of combination;
10.142 of neutral nitrate, contain oxide 6.014 3 atoms.
5.493 of subnitrate . . . . 4.471 2 ——

3 atoms of neutral nitrate have then let fall 2 atoms of oxide and 1 atom of acid. There remained in solution 1 atom of oxide and 5 atoms of acid.

16.319 of protonitrate of mercury, well dried, gave 2.491 of subnitrate, containing 2.226 of oxide.

These 16.319 contain 12.972 of oxide. Hence 12 atoms of neutral nitrate afforded 1 atom of subnitrate. The water retained 10 atoms of oxide and 11 atoms of acid.

17.950 of deuto-nitrate of mercury, containing 12 of oxide, yielded with cold water 4.622 of subnitrate, containing 4.112 of oxide.

2 atoms of oxide	4.112	precipitated;
4	8.224	left in the solution.
Total	12.336	

There has been deposited, therefore, 1 atom of sub-nitrate, while 4 atoms of oxide and 11 of acid have been dissolved.

We perceive that the acidulous nitrate of bismuth is formed of oxide 1 atom, acid 5 atoms; that of protoxide of mercury, of oxide 10 atoms, acid 11 atoms; that of the deutoxide, of oxide 4 atoms, acid 11 atoms; that is to say, of a quantity of acid in excess over the neutral nitrate, which is by no means proportional to that which constitutes the neutral salt or subsalt; for they would all be composed as follows :—

Nitrate of Bismuth.

	Neutral.	Acidulous.
Oxide	1 atom	1 atom
Acid	2 ——	5 ——

Proto-nitrate of Mercury.

Oxide	1 atom	10 atoms
Acid	1 ——	11 ——

Deuto-nitrate of Mercury.

Oxide	1 atom	4 atoms
Acid	2 ——	11 ——

If it be considered, moreover, that water alone, in saturating the nitric acid, can decompose the sub-deutonitrate of mercury, it will appear evident that it is not acidulous nitrates which are here formed, but a combination of neutral nitrate, water and acid; where the acid acting at once both on the water and on the neutral nitrate, prevents this from being subsequently decomposed by a new quantity of water.

On the above statements of M. Grouvelle, we have only to remark, that the formation of his salts, does not seem sufficiently determinate to warrant all his atomical inferences. We do not believe, for example, in the existence of a compound of ten atoms of oxide of mercury, and eleven atoms of nitric acid.—*Annales de Ch. et de Phys.* xix. 137.

Analysis of a Salivary Calculus of a Horse, and of the Saliva of the same Animal, by M. J. L. Lassaigne.—This concretion had a cylindrical form, resembling that of an elongated ellipsoid. Its length was forty-seven millimetres, and its diameter eighteen. It was as hard as marble. It consisted of concentric coats, without any central nucleus of foreign matter. Its constituents were

Carbonate of lime	84
Phosphate of lime	3
Animal matter	9
Water	3
Loss	1
	100

This result, along with the others already known, demonstrate that the salivary *calculi* found in herbivorous animals, are generally different from those which occur in man, which consist of phosphate of lime, with a little animal matter.

The saliva of the horse, when evaporated at a moderate heat, leaves $3\frac{1}{2}$ parts in the hundred of fixed principles, which are composed of the following ingredients;

Animal matter soluble in alcohol;
Animal matter soluble in water;
Albumen;
Traces of mucus;
Muriates of soda and potash;
Free soda;
Carbonate of lime;
Phosphate of lime;

The saliva of the horse thus presents some differences from that of man, analyzed by M. Berzelius. It contains more albumen and carbonate of lime, and much less mucus. This anomaly may, perhaps, be ascribed to the different mode of obtaining the saliva for analysis. That of the horse was procured by dissecting with care the salivary canal, and receiving the liquid, which flowed out in abundance while the animal was eating. The quantity obtained by this operation, might be estimated at about half a litre.—*Ann. de Ch. et de Phys.* xix. 174.

On the Triple Chloride of Gold and Sodium, by M. Figuier, apothecary at Montpellier.—This is the medicinal preparation, which Dr. Chrestien, of Montpellier, has been in the habit of employing. If to a solution of chloride of gold, made with two ounces of this metal, we add four drachms (about 300 grains troy) of decrepitated sea salt, and evaporate, we obtain a salt perfectly crystalline, containing always the same proportions of gold, which, far from attracting humidity, like the simple chloride of gold, is on the contrary nearly inalterable in the atmosphere, and not liable to change its nature by repeated crystallizations. M. Figuier determined the quantity of gold in this salt, by throwing it down with sulphuretted hydrogen; he found the muriate of soda present, by evaporating to dryness the supernatant liquid of the preceding experiment; and thirdly, he ascertained the proportion of chlorine, by nitrate of silver. By this process he found, that the chloride of gold and sodium contained,

Chloride of gold	69.3
Chloride of sodium	14.1
Water	16.6
	100.0

He then shews that calling the atom of gold 248.6 with Berzelius, a triple chloride which would be formed of one atom of chloride of gold, one atom of chloride of sodium, and eight atoms of water, would have for its constituents,

Chloride of gold, one atom	380	70
Chloride of sodium, one atom	73	13.4
Water, 8 atoms	11.2	16.6
		100.0

Ann. de Ch. et de Phys. xix. 177.

On a new Compound formed by mixing together a solution of Cyanide of Mercury with a solution of Iodide of Potassium by M. Calliot.—In trying to detect the presence of cyanide of mercury, by the iodide of potassium, he was much surprised to see form in the liquid a multitude of white pearly crystals, instead of a precipitate of deutiodide of mercury, as he expected. After having washed these crystals, he dissolved them in water, and re-crystallized the solution. He obtained large plates, thin and brilliant, unalterable in the air, inodorous in the dry state, but having a smell like that of bitter almonds when in solution; soluble in sixteen times their weight of water, at the ordinary temperature, but requiring much less hot water for their solution. They are also soluble in about ninety-six parts of alcohol, at 34° (0.847.).

This new compound, at a temperature incapable of destroying it, loses nothing of its weight, or its lustre. It is of consequence, probably anhydrous. When more strongly heated, it is decomposed, and gives for products, cyanogen, mercury, and a greenish yellow vapour, mingled with protiodide of mercury. The iodide of potassium which remains fixed is blackened by a little finely-divided charcoal. Put successively in contact with the strongest and the weakest acids, such as the benzoic, camphoric and arsenious, it is converted into deutiodide of mercury, and hydrocyanic acid is disengaged. This acid itself, and carbonic acid, produce no effect on the compound. By sulphuretted hydrogen, black sulphuret of mercury is thrown down, and hydrocyanic acid is disengaged.

The hydro-sulphurets produce on the solution of this body, a black precipitate; the salts of lead, a yellow precipitate of iodide of lead, and the salts of deutoxide of mercury, a red precipitate of deutiodide of mercury. Chlorine and the bi-chloride of mercury determine in it a red precipitate soluble in an excess of the solution. Iodine dissolves in it, insomuch the greater quantity, as its solution is the more concentrated. Potash, soda, and ammonia, whether free or combined with an acid, produce no decomposition of it.

M. Caillot had regarded this compound as formed of one proportion of cyanide of mercury, and one proportion of iodide of potassium; but having added sulphuric acid in slight excess to the solution, he found in the liquid, resting over the abundant precipitate of deutoxide of mercury which took place, a small quantity of cyanide of mercury. It remains, therefore, to make the analysis of it, in order to learn its composition. He concludes, by stating that 24 parts treated by hydrosulphuret of soda, afforded nearly 13 of sulphuret of mercury.—*Ann. de Ch. et de Phys.* xix. p. 220.

On the Zëine of Maize.—The *zëine* of John Gorham, is obtained from Indian corn, by infusing it in water, filtering, and treating with alcohol the matter insoluble in the former liquid, and evaporating the alcoholic solution. We thus obtain a yellow substance having the appearance of wax; it is soft, ductile, tough, elastic, insipid, nearly void of smell, and denser than water. When heated, it swells, becomes brown, exhales the odour of burned bread mixed with an animal smell, and leaves a bulky charcoal. It affords no ammonia. Insoluble in water, it dissolves readily in alcohol, oil of turpentine, sulphuric ether, and partially in the mineral acids, and caustic alkalis. It is insoluble in the fixed oils, but may be united with the resins.

Though different from all known vegetable products, it approaches in its nature somewhat to gluten, from which, however, it is distinguished by the absence of azote; by its permanence; for it does not change its nature, or become spoiled in the air; and, finally, by its solubility in alcohol. It resembles the resins in this respect, since it dissolves like them in the volatile oils, *&c.* It is inflammable, and consists of carbon, hydrogen, and oxygen. It is very readily extracted from the maize, by digestion for a few hours in hot alcohol. The filtered liquid, yields, on evaporation, pure zëine.

On the Hop. By M. M. Payen and A. Chevalier.—It appears that M. Planche, some time ago, ascertained that the three active ingredients of hop, the oil, resin, and bitter principle reside in the brilliant yellow grains scattered over the calicinal scales of the cones, which serve as their envelope. Dr. Yves, of New York, and the authors of the present memoir, now confirm this position. This matter, when insulated, is of a golden yellow colour, in little grains, formed of an impalpable powder, without consistence, which attaches itself to the fingers and renders them rough; it has a penetrating aromatic odour. 200 grammes of this substance being put into a retort, with 300 grammes of distilled water, the mixtnre was subjected to distillation, and afforded water and oil of an odour entirely similar to that of this yellow matter, but much more penetrating, narcotic, and very acrid in the throat. It was not easy to determine the weight of this essential oil, although it was in considerable quantity, because it is very volatile, soluble in a great measure in water, and adhering to the sides of the globular receiver. However, from the weight of what they could collect, and by making an approximate estimate of the rest, they imagined that the total amount of the oil was 4 grammes, or 2 *per cent.* of the yellow matter employed; and as this yellow matter is contained in hop, in the proportion of $\frac{1}{10}$, it follows that the hop contains about 0.002 of essential oil.

The water, over which the oil floated, had the same flavour as

the oil itself, but less strong. After drawing it off, and leaving it for a few days, it became alkaline, and its acrimony disappeared. They found that the alkaline properties were owing to a subacetate of ammonia. The following are the ingredients they extracted from the 200 grains of this yellow substance:

1 Water;
2 Essential oil;
3 Carbonic acid;
4 Subacetate of ammonia;
5 Traces of osmazome;
6 Traces of fatty matter;
7 Gum;
8 Malic acid;
9 Malate of lime;
10 Bitter matter, 25 grammes;
11 A well characterized resin 105 grammes;
12 Silica 8
13 Traces of carbonate, muriate, and sulphate of potash;
14 Carbonate and phosphate of lime;
15 Oxide of iron and traces of sulphur.

The resin is of a golden yellow colour, passing to orange red on exposure to the air. The bitter matter, when dried, is white, yellowish, attracts slightly the humidity of the air; put in the mouth it causes, as its name indicates, a bitter taste; taken inwardly in very small quantity, it destroyed the digestive faculties, depriving the person of all appetite for food; an action which lasted from eight to ten hours. It occasions no narcotic feeling, as the essential oil appears to do; it is soluble in water, ether, alcohol, communicating to these liquids its bitterness. With re-agents it presents the following phenomena:

With acetate of lead . .	no change,
Subacetate of lead . . .	0
Nitrate of cobalt . . .	slightly troubled,
Muriate of platinum . .	a light precipitate, insoluble in even a large quantity of water,
Nitrate of silver	a slight mottling,
Infusion of nut galls . .	0
Nitrate of mercury . . .	mottling,
Muriate of tin	slightly turbid,
Bichloride of mercury . .	white precipitate,
Sulphate of iron	slight turbidity,
Nitrate of copper . . .	slight, flocky precipitate.

The resin is soluble in alcohol and ether, colouring these *menstrua* to a golden yellow. The solution yields, by evaporation, a resin which, when detached from the capsule, is in fine

yellow scales, of a perfect transparency. Put in the mouth, it imparts to the palate a bitter taste; distilled water boiled on it acquires this taste, without any colour. With the weak acids it experiences no alteration. It is dissolved by the alkalis; and the acids precipitate it from this solution. The above results were obtained apparently from English hops.

According to Doctor Yves (*Annals of Phil.*, March, 1821,) 120 grains of the yellow grains, treated with different menstrua, afford—

Tannin	5 grains
Extractive matter	10
Bitter principle	10
Wax	12
Resin	36
Fibrous or ligneous residuum	46
	119

M. Planche, one of the Editors of the *Journ. de Pharmacie*, in commenting on the above Memoirs on the Hop, thinks himself warranted, from his own experiments, to consider the analysis of M. Payen more exact than that of Doctor Yves. We are promised in a future Number some remarks on the medicinal properties of the above yellow matter of the hop.

On the volatile Oil of Bitter Almonds as a Poison. By M. Vogel, *of Munich.*—To deprive the oil obtained from bitter almonds by distillation, of its hydro-cyanic acid, he agitated it with a concentrated solution of potash, and distilled it to dryness. The oil volatilized with the water, and the residuum in the retort contained cyanide of potassium. To be certain that the oil was entirely deprived of all its hydro-cyanic acid, he distilled it anew with potash; but this time the residuum contained no cyanide of potassium.

The volatile oil of bitter almonds, thus purified, is without colour, and heavier than water. Its taste is extremely acrid and burning; it crystallizes rapidly by contact of air; it dissolves easily in alcohol and ether, but only in very small quantity in water. The flame of its combustion is very brilliant, and accompanied with much smoke.

In order to find whether this oil, freed from its hydro-cyanic acid, was still poisonous, M. Vogel put a drop of it on the tongue of a sparrow; it died, after violent convulsions, in a few seconds. He poisoned a dog, two months old, with four drops of it. He hence infers, that this volatile oil, well purified, produces on animals deleterious effects, analogous to those of the hydro-cyanic acid, although in a feebler degree.

Analytical examination of two species of Flour, designated under the names of Wheat Flour of Odessa, and French Wheat Flour. By M. Henry.—The Odessa flour was of a dirty yellowish hue; with scarcely any direct taste, but leaving in the mouth a powdery after-taste ; its smell was not unpleasant, yet approaching to that of dust. It was somewhat rough to the touch, of little unctuosity, and contained many small yellowish points. The French flour was of a fine white, a fresh smell, an agreeable taste, and softer than the other to the touch.

A paste was made with each of these flours and water. The Odessa specimen absorbed 60 parts of water *per cent.;* the French 45 parts. The Odessa paste had a dirty yellowish aspect, it was elastic and tough ; when bruised between the teeth, it developed a bitter taste. The paste of the French flour was greyish white, elastic, less tenacious than the former, and of a sweet taste.

These doughs were separately washed under a slender stream of water, kneading them continually in the hands. By this means all the gluten was obtained, which was well washed, weighed, dried in a stove, heated to 40° centigrade (104° Fahrenheit), and weighed anew. It lost thus two-thirds of its weight. The gluten obtained from the Odessa wheat, weight 36.5 when fresh, and 12 in the dry state. This gluten had a greyish aspect, was very elastic, very tough, and appeared to be of a very good quality. The gluten extracted from the French flour weighed 24.5 fresh, and 8 dry ; it was greyish, elastic, and tough. It remained in water without changing, for a much longer time than that of the Odessa flour.

The water of the washings of the flours, containing the starch, was filtered in order to separate this principle, which washed, dried, and weighed, presented the following characters for each flour :

The starch of the Odessa wheat was greyish white, rough to the touch, and gritty under the teeth ; it weighed 66. That of the French wheat was more decidedly white, and less harsh to the touch ; it weighed 70, the water of the washings, afforded no blue precipitate with tincture of iodine ; it was slightly opaque. The water from the Odessa wheat had a bitterish taste, not found in that of the French. This water, exposed to a gentle heat, deposited a matter which was recognised to be albumen. That of the Odessa washings evaporated to dryness, at the heat of a salt-water bath, afforded a residuum of a reddish-brown, and the other of a yellowish-brown. These *residua* were viscid, and a little saccharine. That of the Odessa grain was also slightly bitter. Treated with water to separate the albumen, the liquid was evaporated anew to the consistence of an extract.

The extractive matter was now subjected to the action of alcohol at 40° (8.817,) in order to separate the portion of sugar which it contained. The quantity of sugar furnished by each was nearly the same; but that of the Odessa specimen was coloured and faintly bitter.

The residuum insoluble in alcohol, was digested with water, and concentrated. It became viscid, was somewhat whitish, without any marked taste, and a little coloured. Silicated potash yielded a precipitate, indicating the presence of gum. To determine what salts are contained in these flours, they took 100 parts of each, and calcined them slightly in a platinum crucible. The carbonaceous residuum was pulverized, and treated with boiling water; this filtered and evaporated to dryness, gave for each flour about 0.15 of saline matter, and a minute portion of silica.

On examining the bread manufactured with these two kinds of flour, the French was found to be the sweetest, but the other kept longer fresh.—*Jour. de Pharm.* Feb. 1822.

On Carbon considered as a discolouring substance, by M. A. Bussy.—This memoir, to which the Pharmaceutical Society of Paris has very properly adjudged the first prize, is preceded by general considerations on colouring matters, which the limits of our Journal do not permit us to detail. M. Bussy, in order to shew that the action of a chemical body varies much according to the physical state in which it occurs, quotes the fact that gelatinous alumina deprives of colour decoctions of almost all dyewoods, whilst it has no action on them, when it has acquired cohesion by desiccation. On adding likewise to a liquor, a solution of acetate of lead, we destroy the colour by occasioning a precipitate, which carries with it a portion of the colouring matter, whilst the same precipitate in the dry state would exercise no action on it.

M. Bussy's first chapter treats of the manner of comparing together the discolouring or blanching powers of different charcoals. The solution of indigo in sulphuric acid appears to him the best means to be employed for establishing in an exact and comparative manner, the discolouring power of different charcoals. 1st, Because we can always know the exact quantity of colouring matter on which we act, from the weights of indigo dissolved, and water of dilution. 2d, Because the marked hue of indigo does not suffer us to hesitate concerning the blanching of the liquor, when it occurs. 3d, Because indigo is less susceptible than any other matter of being altered by light, heat, *&c.*, which might exercise some influence on the results.

It might, indeed, be properly objected, that the acid of the solution may act upon the substances foreign to the charcoal. To avoid this inconvenience, M. Bussy took a solution made

neutral in the following way:—Into an acid solution of indigo, he put a certain quantity of wool; when it had taken up all the colour of which it was capable, he withdrew it, washed it with cold water, to remove all the loosely-adhering indigo; after which he boiled it in water containing an excessively small quantity of potash; merely what he supposed necessary to saturate the acid which the wool might still retain. In this manner a neutral solution was obtained, whose proportion of indigo could be ascertained; for it is known from the very exact experiments of M. Welter, that 100 parts of chlorine in weight destroy the colour of 226 parts of indigo. Hence in taking a solution of chlorine, whose proportions are known, we can determine what is the quantity of the solution of indigo which a known portion of the first decomposes. It was in this way that M. Bussy learned that the solution which served for his trials, contained one thousandth of its weight of indigo.

To try a charcoal with this solution, he took a certain quantity, which he put into a phial, in contact with a known quantity of the charcoal; he heated slightly, which hastens somewhat the discoloration, and he added the test-liquor till the charcoal ceased to discolour it. The discoloration also takes place in the cold, but more slowly.

In his second chapter he inquires what among the different substances, contained in charcoal, are those which act efficaciously in discoloration. He began by trying different species of charcoal obtained by calcination in close vessels, at a heat sufficiently strong to get rid as much as possible of the gaseous bodies which it usually evolves. He thus calcined wood, starch, gelatine, gum, blood, coal. All these species of carbon were more or less hard, friable, brilliant, and *did not sensibly discolour the test-liquor.* He next examined bone black, usually employed for the clarification of sugar in France; and he examined particularly the charcoal, known under the name of the charcoal of Prussian blue, which is produced by the calcination of animal matters along with potash. To procure this last product he took dried ox-blood, which he calcined twice over with its own weight of subcarbonate of potash, at a temperature a little under a dull red; he obtained a spongy mass, which, after being sufficiently washed with boiling water, to separate every thing soluble which it contained, left a charcoal of a dull black hue, excessively light and spongy, whose discolouring power, compared to that of bone-black, was as 40 to 1. Astonished at so great a difference, he wished to see if the charcoal had not contracted some new combination, or if it owed this exaltation of property to the presence of a foreign body, and particularly to potash. After having washed it as well as possible in boiling water, which removed the ferro-prussiate of potash, as well as the sulphuret and carbonate of this

alkali, the residuum was treated with muriatic acid, which occasioned an effervescence due to the decomposition of a little sulphuret of iron; and the acid being boiled on the charcoal, carried off the phosphate of lime, with a little lime and iron. This charcoal so treated, discoloured still very well; even better than before. It was therefore improbable that this charcoal owed its property to the presence of potash, unless indeed there had been formed some alloy which could resist the action of muriatic acid. To ascertain this point, he calcined the charcoal which had been treated with the acid, but the ashes were not alkaline, and contained no salt of potash; they were a mixture of oxide of iron and silica, forming a twelfth part of the weight of the charcoal calcined.

It is then in the residuum of the action of muriatic acid that the discolouring property resides, and this residuum is composed of charcoal, and iron in the state of a carburet probably, since it is not acted on by muriatic acid, a little silica, which is only accidental, and azote. To determine what might be the influence of the azote, he took calcined blood alone; 3 decigrammes of this blood, burned with peroxide of copper, yielded 12 of azote for 100 in weight of the charcoal. The proportion of azote is very variable; we may even fail to obtain any, if the charcoal has been strongly enough heated; but in all these cases this charcoal is hard, brilliant, and *does not discolour*. That resulting from the calcination of blood with potash, tried by the oxide of copper, contains still a certain quantity of azote, also variable, but much less than the first; but as it is one of the characters of azotized carbon to furnish hydro-cyanate of potash when we calcine it with this alkali, he conceived that by treating this charcoal with a new quantity of potash, he could remove from it still a portion of azote; he treated it anew with potash, and the quantity of azote which after the first calcination was 5 *per cent.*, was reduced after the second to 2; and at the third the charcoal contained no longer a sensible quantity of it. Now since this reiterated action of the potash merely augmented the discolouring property of the charcoal to such a degree, that after the last calcination it was equal to 50, that of bone-black being 1, *it follows that azote is without effect in discoloration; and consequently, it is not to this body that animal charcoal owes its property.*

It remained to know if the discolouring property was not owing to the combination of charcoal and iron, a combination which resists, as we have seen, the action of boiling muriatic acid; but whose presence cannot be denied, since we always find iron in the residuum of the combustion of charcoal. This quantity of residuary iron, however, is not constant, which shews that the combination is not in any fixed proportion. It is observed, that the quantity of iron is so much greater as the heat was stronger, at which the potash and blood were calcined; but

on the contr ry, if we conduct the heat properly, and repeat the calcination with the potash a sufficient number of times, the residuum will contain but a very minute quantity of iron.

The cause of this difference is easily perceived. We know that when we treat animal matters, and especially blood, with potash, we obtain ferro-prussiate of potash, since on this product is founded the manufacture of Prussian Blue ; but ferro-prussiate of potash contains hydrogen, carbon, iron, and azote. The three last substances are derived from the elements of the blood, which combine together so as to give birth to the ferro-prussic acid ; consequently the more we shall favour the formation of the ferroprussiate of potash, the more iron we shall carry off from the blood.

But if we apply all at once a too violent heat, then no more ferro-prussiate of potash is formed ; for this salt is decomposable at a very high temperature, and gives as products of its decomposition a little hydrocyanate of potash, and a solid compound of iron and charcoal. In this case the azote of the blood is dissipated, and the iron remains entire, combined with the charcoal. M. Bussy succeeded by several successive calcinations of blood with potash, to deprive it of all the iron which it contained. The greatest difficulty experienced to attain this point depends upon this, that the azote abandons more easily the charcoal than the iron does, and that when we have arrived at the point where the charcoal contains no more azote, potash is then without action on the iron. But if at this period we add blood-charcoal, or any animal substance containing much azote, as an empyreumatic animal oil, then we succeed readily in carrying off the residuary portion of iron, and we obtain a charcoal which burns without residuum. Albumen and gelatine, substances which contain much less iron than blood, are very readily brought to the point of containing none of it. For this purpose it is sufficient to dry them, to reduce them into powder, and to calcine them with the potash of commerce. We must likewise take care after every calcination to treat the residuum at first with water, then with muriatic acid, which removes every time a small portion of iron, which is probably not combined with charcoal.

Since in all these successive treatments to which we subject charcoal to deprive it of foreign substances, it loses none of its discolouring power, we must conclude that this property resides essentially in charcoal, but that it is developed in virtue of the physical circumstances in which it is placed. This conclusion which rigorously follows from the experiments just adduced, is supported by a multitude of others, of which a few may be detailed; and which were made chiefly with the intention of shewing that the discolouring property of charcoal varied according as the substances with which we calcine it, act with more or less energy on it, and on the foreign bodies which it contains.

We have seen that blood, calcined alone, yielded a charcoal, which does not discolour, because its particles contract so strong an aggregation, that they are not susceptible of combining with the colouring matters. But if instead of calcining the blood alone, we calcine it mixed with an inert substance, with phosphate of lime for example, which does nothing but oppose the aggregation of the particles, we obtain a charcoal susceptible of discolouring, and whose blanching power will be represented by 12, assuming always for unity, the blanching power of bone-black. If we calcine blood with very fine carbonate of lime, its discolouring property becomes 18; finally, if we treat it with potash, it may even rise to 50. It is needless to say, that in these different trials, the charcoal was deprived of the potash, chalk, or phosphate of lime, with which it had been calcined.

We may easily understand all these results, by considering that the first substance, phosphate of lime, acts simply by operating the division, nearly mechanical, of the charcoal; the second acts already with a little more energy, for we observe that the water of edulcoration contains a small quantity of ferro-prussiate of lime; finally, the potash not only attenuates the particles of the charcoal, but by combining with all the foreign principles which it contains, it must leave it in a certain state of porosity, which M. Bussy regards as more favourable to discoloration than division itself.

In order to ascertain that charcoal truly enjoys the property of discolouring, independently of the substances which are made to act upon it, M. Bussy sought to procure pure charcoal, by means of the decomposition of subcarbonate of soda with phosphorus, and he succeeded in obtaining a sufficient quantity to determine its discolouring power, which he found to be equal to 12.

He next tried lamp-black; but in its natural state it contains resinous matter, which prevents us from regarding it as pure charcoal. To make it such, we must calcine it anew. In this state it is capable of discolouring; its blanching power being expressed by 4; and although it is obviously less than that of the preceding, it is enough to shew that the property resides in charcoal, and that it may vary from 4 to 12, by the mere effect of physical circumstances, very difficult to appreciate; for it could not be easy to establish a perceptible difference between these two charcoals. If instead of heating the lamp-black by itself, we mingle it exactly with 15 or 20 times its weight of pure carbonate of potash (that from calcined tartar), and expose it to a very strong heat in a platina crucible, it is observed that the mass becomes hard, compact, experiencing incipient fusion in the parts contiguous to the crucible. If we dissolve this mass in water, in order to separate all the pot-

ash from it, the charcoal which remains has a discolouring power, expressed by 15; and this charcoal contains no foreign body, for it burns away without leaving any residuum. Besides, we cannot here suppose an alloy of charcoal with potassium, since these kinds of alloys are decomposed by water; whilst the discolouring property, continues still in all its intensity, after the action of muriatic acid. We are, however, led to conclude, that potash may at a high temperature act upon finely divided charcoal, so as to change its physical constitution; and that this change is the sole source of the increase of its blanching power, since it has contracted no combination with it. M. Bussy attempted to substitute caustic potash for the subcarbonate, to see if the effect would be greater; but as the caustic potash contains much water, the charcoal decomposed this; and was thus entirely transformed into carburetted hydrogen gas, and into carbonic acid gas, which combined with the potash.

We now readily perceive how by calcining a vegetable substance with potash, we come to obtain a discolouring charcoal; and in fact, by calcining starch with four parts of potash, a charcoal is obtained, whose blanching property was expressed by 10. The charcoal resulting from the calcination of the acetate of potash, possesses a discolouring power of 3. And we see also, that if the discolouring force of these charcoals is not so great as that of the charcoal obtained from the calcination of animal matters, this depends on the circumstance, that in these last, there are two causes, which concur to augment the blanching power; 1. the action of the potash on the charcoal itself; and 2. the abstraction which it effects of the foreign matters, such as the iron and azote, whilst in the vegetable matters, the first effect is the only one which occurs.

Animal matters themselves do not all yield a charcoal equally good for discolouring; but it is so much the better, as the potash has combined with a larger quantity of the principles of the animal substance; hence, the more prussiate of potash a body affords, when calcined with this alkali, the better will its charcoal discolour. Gelatine and albumen, which yield much less Prussian blue than the blood does, furnish a charcoal, whose discolouring force is from 36 to 40, whilst that of blood amounts even to 50.

Let us now endeavour to explain the mode of action of bone-charcoal, which is most in use, at the same time that it is one of the most complex in its composition. This charcoal, such as we find it in commerce, varies in its composition; but in general we find it to contain—

Phosphate of lime, Carbonate of lime, Sulphuret of lime, Sulphuret of iron, Oxide of iron . .	88
Iron in the state of a silicated carburet	2
Charcoal, containing 6 or 7 *per cent.* of azote	10
	100

When we treat this charcoal with muriatic acid, it leaves a residuum of about twelve per cent., which is a mixture of azotized carbon, of silica and carburet of iron, so that the pure charcoal appears to constitute not more than one tenth of the primitive charcoal. It would consequently appear that since the discolouring power resides in the carbon, that of this purified carbon ought to be ten times more considerable than that of the crude charcoal; but this is by no means the case, for the discolouring power of the purified charcoal is only 1.5, that of the crude being 1. To account for this apparent anomaly, we must consider, as will be shewn in the next chapter, that when carbon blanches a solution, the colouring matter comes to be deposited on its surface; of consequence, the more surface the charcoal presents to the solution, the more easily will the discoloration occur. It is precisely the same effect, which is observed in dyeing fine or coarse wool; more colouring matter being required for the first, than for the last, in equal weights. Hence, when we treat bone black with muriatic acid, its discolouring force is found to be increased in the ratio of the foreign bodies which we abstract, and diminished in the ratio of the extent of surface which it loses; so that it is the relation of these two changes which determines the increase or diminution of its properties. None of the substances present in bone-black when used by itself, except the charcoal, discolours.

After establishing the difference which exists between the blanching property of different charcoals relative to the solution of indigo, it became interesting to know, if this relation would be preserved for the different colouring matters. M. Bussy made a test liquor with melasses; it was composed of one part of melasses, diluted with twenty of water, in which he tried the different charcoals marked in the following table. The quantities acted on were more considerable than those indicated in the table, but he reduced them to one gramme for the sake of comparison.

From this table we perceive; 1st. that the same quantity of the same charcoal discolours about ten times more of the indigo liquor, than of that of melasses; 2d. that the different charcoals preserve, in reference to these two substances, the same order in their blanching power, but that the proportion which exists between them, when they are tried by the melasses, is less than

when they are tried with the indigo. Thus in the case of indigo we find that the blanching power of blood-charcoal is to that of bone-black as fifty to one; and for the melasses, this proportion is twenty to one. To find out what might be the cause of this variation, he made the same trials on decoctions of cochineal, logwood, gum, *&c.*, and he observed that it always required more charcoal to blanch the same bulk of liquor, the more the relation between the discolouring power of the charcoals diminished; that is to say, if we suppose, that we take the like volume of all the test-liquors, that which will require least charcoal for its discoloration, is that which will establish the greatest difference between the discolouring force of the different charcoals.

Table of the comparative discolouring power of different Charcoals.

SPECIES OF CHARCOAL.	Weight.	Indigotest-liquor consumed.	Melasses liquor consumed.	Blanching power by the Indigo test.	Blanching power by the melasses test.
Blood calcined with potash	1 gramme.	1 litre 6.	0.18	50	20
Blood calcined with chalk	id.	0.57	0.10	18	11
Blood calcined with phosphate of lime . .	id.	0.38	0.09	12	10
Gelatine calcined with potash . . .	id.	1.15	0.14	36	15.5
Albumen calcined with potash . . .	id.	1.08	0.14	34	15.5
Starch calcined with potash . . .	id.	0·34	0.08	10.6	8.8
Charcoal of acet. potash	id.	0.18	0.04	5.6	4.4
Charcoal from subcarb. soda by phosphorus .	id.	0.38	0.08	12	8.8
Calcined lamp black .	id.	0.128	0.03	4	3.3
——— with potash .	id.	0.55	0.09	15.2	10.6
Bone-black treated with muriatic acid and potash	id.	1.45	0.18	45	20
Bone-black treated with muriatic acid . .	id.	0.06	0.015	1.87	1.6
Vegetable or animal oil calcined with phosphate of lime . .	id.	0.064	0.017	2	1.9
Crude bone-black . .	id.	0.032	0.009	1	1

Since the Indigo liquor contains the thousandth part of its weight of Indigo; that is, one gramme per litre, we perceive that the numbers, which express the quantity of liquor blanched per litre, express the real quantity of Indigo fixed on the charcoal.

Chapter III. On the Mode of Action of Charcoal in discolouring.

It is generally thought that the charcoal acts on colouring matters, by decomposing them; and this opinion is founded on it having been observed, in treating different matters with charcoal, such as beer, mucilage, melasses, wine, *&c.*, that the discoloration was accompanied with a disengagement of gas. Presuming that this opinion was entirely erroneous, and that to the decomposition of the colouring matter, a disengagement of gas had been improperly ascribed, which must depend on some other cause, M. Bussy made the following experiments: He took 8 ounces of melasses, which he diluted with a pound and a half of distilled water, agitated it with carbonate of lime to neutralize the acids which might act on the charcoal; then filtered the liquid, and heated it to expel the gases which might be held in solution in the water.

On the other hand, he took 1 ounce of blood-charcoal very carefully washed; boiled it in 8 ounces of water, to expel all the air which might be adhering to it, and when he judged it to contain no more air, he added the melasses, so that the vessel which he employed, was perfectly full. The apparatus being thus arranged, and perfectly freed from air, he engaged the extremity of the tube, under a graduated tube filled with water; and he heated the vessel in the salt-water bath for two hours, at a temperature approaching to that of boiling water. No gas whatever was disengaged, and the liquor became completely colourless; the experiment succeeds equally well in the cold. He tried also cochineal, and several other colouring matters, and he obtained no evolution of gas. As to wine, we must take great care to employ a charcoal which has been previously treated with muriatic acid, for otherwise, the acids which it contains, decompose the carbonate of lime which is present in the charcoal, and gives rise to a disengagement of gas.

The colouring matters not being decomposed by the charcoal, as appears from this experiment, we conceive they must fix themselves in the charcoal, augment its weight proportionally to the quantity of liquor discoloured; and that, in some circumstances, we may re-produce the solution of the colouring matter.

To verify this conjecture, he took three species of charcoal, which he knew acted with a different intensity; these were: 1st, charcoal of blood; 2d, bone-black, purified; 3d, crude bone-black. These charcoals after having been washed with boiling water, to separate all their soluble ingredients, were dried at the water-bath, for the same space of time. He then took 5 grammes of each of these charcoals, which he heated separately with 20 grammes of melasses, diluted with 4 parts of water. This liquor was left in contact with each of the charcoals during the same time; at the end of which they were fil-

tered, and were found differently discoloured, according to the species of charcoal which had acted upon it. Each of these charcoals having been washed, and then dried at the water-bath, taking every precaution to place them in the same circumstances, it happened that

The blood-charcoal was increased in weight by	1.56 gr.
The purified bone-black	0.54
The crude bone-black	0.3

This increase of weight, which is, as we see, relative to the blanching power of each charcoal, indicates that the discoloration is effected by the combination of the colouring matter with the charcoal. The same effect is also remarked, when we employ indigo; but it is much less appreciable. We have reason, indeed, to be surprised at the considerable augmentation of weight which charcoal assumes in the discoloration of melasses; but it is probable, that this increase proceeds not only from the colouring matter, if there exists a particular one, but also from the mucilaginous matters, and others which are not in perfect solution. It is a fact, well known to refiners, that the waters of the charcoal washings which have served for clarification, are very mucilaginous. If, instead of taking cold water to wash this charcoal, boiling water be taken, slightly alkalized with potash, the water comes off coloured of the melasses tint, which further confirms the conclusion to be drawn from the augmentation of the weight of the charcoal, namely, that the colouring matter combines with the charcoal, without experiencing decomposition.

But this truth seems to be established in an incontestable manner, by an experiment which M. Bussy made on indigo.

Into a solution of indigo by sulphuric acid, made with one part of indigo, and 7 parts of acid, the whole diluted with 92 of water, he added a certain quantity of charcoal which completely discoloured it. The liquor thrown upon a filter, passed through with a slight tint of yellow, visible only in a great mass of the liquid. The filter was washed with a small quantity of cold water, to remove what of the solution remained adhering to the filter; this water passed colourless. He threw afterwards on the same filter, boiling water, containing a very small proportion of sub-carbonate of potash; instantly the liquor passed through of a blue colour, not of a feeble and uncertain tint, but with an intensity of shade, equal to that of the first solution. It is possible, by continuing the washing with very hot alkalized water, to remove from the charcoal almost the whole of the indigo which it contains.

If we take again this same washed charcoal, and put it in contact with the blue liquor of the washings, after rendering it slightly acid by the addition of a little sulphuric acid, it will discolour it anew, and the indigo fixed in the charcoal may be

once more carried off by the potash, so that we may make it pass in succession a considerable number of times, from the charcoal to the solution, and from the solution to the charcoal. It is, however, observed, that these successive solutions terminate in altering the indigo; and, at each new discoloration, the liquor retains a faint yellowish colour, which indicates the decomposition of a small quantity of indigo.

We may conclude from all that we have seen, that the discolouring property is inherent in charcoal; but that it becomes manifest only when the charcoal occurs in certain physical conditions, among which porosity and division hold the first rank; that no charcoal can discolour when it has been heated strongly enough to become hard and brilliant; that all, on the contrary, possess this property when they are sufficiently divided, not by a mechanical action, but by the interposition of some substance which opposes their aggregation; and that the superiority of animal charcoal, such as that of the blood and geltaine, proceeds especially from its great porosity.—*Jour. de Pharm.* June, 1822.

Analysis of the Bark of the Quassia Simarouba of Linnæus. By M. Morin.

1. A resinous matter;
2. A volatile oil having the odour of benzoin;
3. Acetate of potash;
4. An ammoniacal salt;
5. Malic acid, and traces of the Gallic;
6. Quassine;
7. Malate and oxalate of lime;
8. Some mineral salts, oxide of iron, and silica;
9. Ulmin and ligneous matter.

Analysis of the Smelt (Salmo Eperlanus of Linnæus). By M. Morin.

1. Albumen;
2. Mucus;
3. Osmazome;
4. Muriate of ammonia;
5. A gelatinous matter;
6. Muriate of potash.
7. Phosphates of potash, magnesia, iron, lime;
8. Carbonate of lime;
9. An oily matter;
10. Phosphorus;
11. Animal fibre.—*Jour. de Pharm.* Feb. 1822.

New Researches on the Composition of the antimoniated sulphuret of Silver, the red Silver of Andreasberg. By M. P. A. Bonsdorff.—The pounded ore was put into an apparatus, simi-

lar to that employed by Berzelius, for the analysis of the ores of nickel (see our last Number), and being heated with the flame of a spirit lamp, was subjected to a stream of hydrogen, evolved, from zinc and dilute sulphuric acid. In this way, the presence of oxide of antimony would be proved by the production of water, as he previously ascertained by a comparative experiment on an artificial compound. But no water was formed from the mineral.

The result of his analysis is :

				Oxygen.	Sulphur
Silver,	0.8865	58.94	which would require	4.36	8.768
Antimony,	0.3436	22.84		4.19	8.423
Sulphur,	0.2498	16.61			
Earthy matter,	0.0045	0.30			
Loss,	1.3100	1.31			
		100.00			

The chemical constitution he represents by $2\,Sb\,S^3 + 3\,Ag\,S^2$, which would give the proportions,

Silver,	58.98
Antimony,	23.46
Sulphur,	17.56
	100.00

Klaproth, in his analysis, had ascribed the loss of weight to oxygen, and gave these proportions of the components,

Silver,	60
Antimony,	19
Sulphur,	17
Oxygen,	4
	100

By adding the weight allowed for the oxygen, to that of the antimony, Klaproth's analysis agrees very closely with M. Bonsdorff's. *Ann. de Chim. et de Phys.* xix. i.

Analysis of the mineral and thermal waters of Mont Dore. By M. P. Berthier.

These waters are among the most celebrated of mineral springs. The Romans had collected them in a vast and sumptuous edifice. They are composed as follows:

	Anhydrous Salts.	Crystallized Salts.
Neutral carbonate of soda	0.0006330	0.0006930
Muriate of soda	0.0003804	0.0003804
Sulphate of soda	0.0000655	0.0001489
Carbonate of lime	0.0001600	0.0001600
Carbonate of magnesia	0.0000600	0.0000600
Silica	0.0002100	0.0002100
Oxide of iron	0.0000100	0.0000100
	0.0015189	0.0016623

This water is distinguished by the large quantity of silica

which it contains. The water deposits this silica in the subterraneous channels which it passes through, under the form of tubercular masses, often very large, and resembling flint. *Ann. de Chim. et de Phys.* xix. 25.

On the influence which water exercises on certain Animal Products. By M. Chevreul.

1. *Tendons.*—These, when dried, become much slenderer, lose their white colour, their satiny lustre, and extreme pliability. They acquire the semi-transparency of horn, a yellow colour, bordering slightly on red. Tendons of the elephant, preserved four years in the dry state, re-produce a fresh tendon, when kept some time immersed in water.

Table of experiments on Tendons.		Water absorbed by 100 parts of the opposite bodies, after immersion of	
100 parts of a large tendon of the elephant, are reduced by drying in the air, to	51.56	12 to 24 hours	102. 0
In vacuo, to	50.00	8 days at least	147. 0
100 parts of a slender tendon of the same—in air, to	46.91	12 to 24 hours	130. 3
In vacuo	43.36	8 days at least	147.68
100 parts of a large ox tendon are reduced in air, to	52.96	12 to 24 hours	100.34
In vacuo, to	49.61	8 days at least	146.58
100 of slender ox-tendon in air, to	44.15	12 to 24 hours	132. 0
In vacuo	42.34	8 days at least	148. 0
100 of slender human tendon become in air	43.13	12 to 24 hours	147.87
In vacuo	37.98	5 days	271.79
2. *Yellow elastic tissue.*—100 parts fresh from the elephant are reduced in air, to	52.57	24 hours	99. 0
In vacuo, to	50. 5	12 days	147. 0
100 parts fresh from the ox—in air	52. 8	24 hours	99. 4
In vacuo to	49. 8	12 days	148. 0

3. *Cartilage of the Ear.*—100 parts of that of a man of forty years, after it was steeped in water, were reduced in the air to 33.5, and *in vacuo*, to 30.64. These 30.64 parts had absorbed at the end of 24 hours, 66.14 of water, and at the end of four days, 69.36, exactly what they had lost by desiccation.

4. *Cartilaginous ligaments.*—100 parts of the cartilaginous ligament of the knee of a woman, thirty years of age, after having been steeped in water, were reduced, in air to 26.41, and *in vacuo* to 23.2.

5. *Of Fibrina.*—100 parts from the arterial blood of a cow were reduced in the air to 21.1, and *in vacuo* to 19.35. 100 parts from the venous blood of a cow, became in air 22.7; *in vacuo* 21.05.

6. *Cornea.*—Of opaque cornea, dried *in vacuo*, 100 parts became, after an immersion in water of

24 hours, 268.18 4 days, 461.28.

7. *Albumen of the Egg.*—100 parts of liquid albumen, after being coagulated, by exposure to air, became 15 parts; and *in vacuo* 13.65. The dry substance was colourless, semi-transparent, and it recovered the properties of boiled white of egg in absorbing water, which, however, it took up only to 68 parts after four days' immersion, instead of the 86.33, which it had lost. White of egg uncoagulated, dries without ceasing to be transparent. It loses in air 85 *per cent.*, and *in vacuo* 86.15. On immersion in water it resumes its original viscid appearance.—*Ann. de Chim. et de Phys.*, xix. 32.

Cure of a Palsy by a stroke of Lightning.—Mr. Samuel Leffers, of the county of Carteret in North Carolina, had been attacked with a palsy in the face, and particularly in the eyes. While he was walking in his chamber, a thunder-stroke threw him down senseless. At the end of 20 minutes he came to himself; but he did not recover the entire use of his limbs till the evening. Next day he found himself perfectly recovered; and he could now write without the use of spectacles. The palsy did not return.

Analysis of the Table-spar of Pargas, the Wollastonite of Haüy. By M. P. A. de Bonsdorff.—The colour of this table-spar is a white, more or less pure; it is translucid on the edges; its lustre is vitreous and moderate; it is semi-hard, scratching glass with difficulty; under the hammer it splits into fine flexible threads. Exposed to the blow-pipe, it melts on the edges at a strong heat, affording a translucent glass, colourless and brilliant. With borax, and the double phosphate of soda and ammonia, it melts into a transparent glass; and gives with soda an opaque globule. With solution of cobalt, it presents a blue colour, a property which does not seem to agree with the nature of tremolite, which treated with the same solution, exhibits usually a flesh colour. Its analysis afforded,

Silica	52.58
Lime	44.45
Magnesia	6.68
Protoxide of iron	1.13
Alumina, a trace	
Volatile matter .	0.99
	99.93

M. Bonsdorff considers it as a bisilicate of lime, or $C\,S^2$.

Analysis of the Meteoric Stone of Juvenas, by M. Laugier.—Four analyses were made; the first by means of acids; the second with potash; the third with nitric acid, to determine the proportion of sulphur; and the fourth by nitrate of barytes; with the view of ascertaining the proportion of potash which M. Vauquelin had detected in this stone. These different ana-

lyses all concurred very nearly as to the proportions of the constituents, which are,

Silica	40
Oxide of iron	23.5
Oxide of manganese	6.5
Alumina	10.4
Lime	9.2
Chromium	1.0
Magnesia	0.8
Sulphur	0.5
Potash	0.2
Copper	0.1
Indispensable loss	3.0
Loss from unknown causes	4.8
	100.0

This remarkable loss, instead of the usual increase of weight, in the analysis of meteoric stones, by the oxidation of the metals, has led M. Laugier to infer that the iron and manganese are here in the state of oxides; and there is no particle of this meteorolite, when pulverized, which is attractible by the magnet. This aërolite seems identical in its composition with the aërolite of Jonzac. The complete absence of nickel, the almost total disappearance of the sulphur and the magnesia, which are replaced by an abundant quantity of lime and alumina, establish between these stones, and the aërolites previously known, very marked differences.

A third example of this peculiar composition is presented in an aërolite, which fell in the neighbourhood of the village of Lontola, in the government of Wibourg, in Finland. M. Nordenskiold, mining engineer at Abo, a pupil of Berzelius, found no nickel in it, and hardly any metallic particles attractible by the magnet.

M. Laugier here concludes with still more certainty than he did in 1820, that chromium is the most constant character of aërolites.

The most numerous and best known meteoric stones have more solidity than these new ones. Globules of iron are found in them, which resist the pestle, and are attracted by the magnet. The latter are friable, easy to pulverize, presenting no obstacles in the mortar, and contain no globules of iron. Their small cohesive force seems to arise from the interposition of different foreign bodies, such as felspar or amphigène. They are, however, less homogeneous in their structure, than those first known. *Ann. de Ch. ét de Phys.* xix. 264.

[The late arrival of the Foreign Journals has prevented us from arranging their contents in a methodical order.]

Art. XVIII. MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

§ Astronomy, the Arts, &c.

1. *Improved Signal for Trigonometrical Measurements.*—A new kind of signal has been used by M. Gauss of Gottingen, in a trigonometrical measurement undertaken in Hanover, which appears to possess many advantages. It consists of reflected solar light: that astronomer having remarked that the light reflected by a small plane mirror, was sufficiently intense to be observed at greater distances than those of the sides of his greatest triangles, had a couple of instruments made which he called Heliotropes, and which, though simpler in their construction than the Heliostat of S'Gravesande, were, like it, intended to reflect the sun's rays in a constant direction. Whilst these instruments were preparing, M. Gauss made use of Hadley's sextant, which, for this purpose, was mounted on a solid foot in the following manner: The plane of the instrument being inclined to the proper degree, the angle between the sun and the terrestrial object to which the sun's rays are to be reflected by the moveable or great mirror of the instrument is to be observed. Then, without altering the apparatus otherwise, the arm which carries the mirror is to be moved, until the index is at double the angle observed, when the sun's rays will be reflected on the spot desired, so that from that point the image of the sun may be seen in the mirror like a star.

The same result may be obtained by previously fixing a third mirror above the moveable mirror, on the same arm, and perpendicular like it to the plane of the instrument; but which makes, with the plane of the great mirror, an angle equal to the complement of 90 degrees of the angle formed by the visual ray with the plane of the third mirror. When, with a sextant thus prepared and fixed on a foot, the distance of the object from the centre of the sun is observed, this third mirror reflects the sun's rays at the same moment on the object itself, and an observer being there, the signal will be the light reflected from the sextant. It is easy, by a little practice, to give that motion to the mirror which is necessary to throw the sun's rays for some time on the same object; for, in consequence of the imperfection of the mirror, the field over which the rays are reflected is large enough to compensate for any little irregularity in the movement.

M. Gauss has found that mirrors, two inches by an inch and a quarter, are quite large enough for these purposes: In some experiments, made with a view of estimating the distance to which

these signals would be visible, a heliotrope and a sextant were placed two geographical miles from each other, the luminous points reflected by those mirrors could be seen by the naked eye, and when viewed through the telescope of the theodolite were too brilliant to give exact points; but when, in place of the sun's light that from a bright cloud was reflected by them, they gave an excellent mark.

At a distance of five miles, the points were still visible, like stars, by the naked eye, and they could be seen through the telescope of the instrument, even in heavy weather, when the great signal itself could not be distinguished. At last the distance was increased to 11 or 12 geographical miles, the stations being at Inselberg and Hohenhagen, and the operators M. M. Gauss and Enke. The light was reflected at intervals by the sextant from Inselberg to Hohenhagen, whilst the light was constantly reflected from the latter to the former place by the heliotrope; these experiments continued 10 days in various circumstances with great success. Each observer reciprocally saw the points at the other station, whilst frequently the mountains on which they were placed could scarcely be seen by the telescope. More than once the light of the heliotrope pierced through mists and even showers of rain.

In general the angles observed by these signals were in greater accordance with each other, than when the ordinary signals have been used.

Baron de Zach proposes a simplification of the reflecting apparatus. He supposes a polyhedral reflector, similar to those sometimes used by bird-catchers, to rotate on an axis passing through the number of its facets, there will then continually be a facet reflecting the light in the proper direction; and farther, such a signal would be visible in all directions. In a trial of this process made in the small way, a piece of rock crystal, cut with many facets, was made to rotate rapidly in the sunshine, it was seen distinctly from all situations at the distance of 2,000 toises.—*Bib. Univ.* xviii. 151.

2. *New Observatories.*—Three new observatories have been established in countries the most remotely situated from each other:—at Nikolajen, on the borders of the Black Sea; at the Cape of Good Hope; and in New Holland.

3. *Astronomical Prize.*—The medal founded by M. de Lalande, to be given annually to the person who, in France, or elsewhere, has made the most interesting observation in astronomy, or rather the mémoire most useful to its progress, has not been adjudged in 1822. So that the prize for 1823 will be doubled, and will consist of a gold medal, value 1,270 francs. It will be adjudged in March 1823.

4. *Cleansing of Orchard Trees by Lime.*—The use of lime has been highly recommended in the dressing of old moss-eaten orchard trees. Some fresh-made lime being slaked with water, and some old worn-out apple-trees well dressed with it with a brush, the result was that the insects and moss were destroyed, the outer rind fell off, and a new, smooth, clear, healthy one formed: the trees, although twenty years old, assuming a most healthy appearance.

5. *Purification of Oil.*—A method of purifying common fish oil, and rendering it equal to the best sperm oil, by the use of animal charcoal, is described as having been discovered in Denmark. The description is very incomplete, but mentions that beef bones which have been boiled, are made into animal charcoal in a peculiar way. The charcoal is mixed with the oil, and repeatedly agitated for two months, after which it is filtered through several strata of charcoal, and used as soon as made. The quantity of gas evolved by the bones in the operation is considerable, and is used for lighting the manufactory and adjacent buildings. The residuum is mixed with clay for fuel. The loss in this process is estimated at 15 per cent., and the gain is equal to 40 per cent., leaving a balance in favour of the discovery of 25 per cent.

The peculiar method of making the charcoal, probably consists in not heating the bones too much. It is well known by the animal charcoal makers in London, that if the temperature be raised too high, the charcoal is worth nothing.

6. *Oil Gas.*—Mr. Wilson proposes obtaining this gas in countries where the oil is chiefly vegetable, by introducing the seeds themselves into the retorts, much in the manner that coals are used here. Besides saving the expense of preparing the oil, it is supposed that the charcoal left may be useful and valuable. We, however, greatly doubt the plausibility of this proposal.

7. *Purification of the Water of the Seine at Paris.*—There is an establishment in this city (Paris) for purifying for domestic use the water of the Seine, which gives constant employment to upwards of 200 persons. The water is pumped into vessels about 20 feet deep, and as many in width, where it reposes 12 hours. The clear water is then raised into another vessel, whence it flows into long and shallow cisterns, on the sides of which a great number of sponge filters are placed, and the sponges are renewed every hour. From the sponge filters it finds its way into square shallow cisterns, each of which has at the bottom a bed of clean Fontainebleau sand, then a bed of pounded charcoal, then another bed of clean sand, and lastly, at top, a bed of coarse river sand, (these last-mentioned filters are renewed every six hours); and

this is the last operation previous to its distribution.—*Tech. Rep.* i. 316. We apprehend this process would not suit the London taste.

8. *Preservation of Steel Goods.*—Mr. Aikin recommends a thin coating of caoutchouc as an excellent preservative of iron and steel articles from the action of the air and moisture; its inalterability, consistence when heated, adhesion to iron and steel, and facility of removal, render it an admirable substance for this purpose.

The caoutchouc is to be melted in a close vessel, that it may not inflame. It will require nearly the temperature of fusing lead, and must be stirred with a horizontal agitator rising through the vessel, to prevent burning. Plates of iron and steel, partly covered with this composition, gave full proof in the laboratory of the protection afforded by the caoutchouc film to the metal.

Mr. Párkins, to whom Mr. Aikin communicated this process, has made much use of it in his blocks, plates, dies, &c. He mixes some oil of turpentine with the caoutchouc, which renders it easily applicable, and leaves the substance, when dry, as a firm varnish impermeable to moisture. This, when required, is easily removed by a soft brush dipped in warm oil of turpentine.—*Tech. Reports*, i. 55.

9. *Preservation of Eggs.*—Eggs may be preserved, according to M. Cadet, a great length of time in lime-water containing excess of lime. An excellent mode of preserving them also, is to place them for about 20 seconds in barley-water; then remove, dry, and put them by. Eggs may be well salted throughout, if laid in brine for 8 or 10 days, and may then be preserved a great length of time.

10. *Fusible Metal, and its application.*—A combination of three parts of lead, with two of tin and five of bismuth, forms an alloy which melts at the temperature of 197° F. In making casts with this and similar alloys, it is important to use the metal at a temperature as low as possible; as if, but a few degrees elevated, the water which adheres to the things from which casts are to be taken, forms vapour, and produces bubbles. Mr. Varley allows the fused metal to cool in a tea-cup until just ready to set at the edges, and then pours it into the moulds, procuring in this way beautiful casts from moulds of wood, or of other similar substances. When taking impressions from gems, seals, &c., the fused alloy should be placed on paper or pasteboard, and stirred about till it becomes pasty from cooling; at which moment the gem, die, or seal, should be suddenly stamped on it, and a very sharp impression will then be obtained.

Mr. Gill has applied the fusible alloy to the formation of metallic pencils. The metal is cast into a proper form in a piece of paper over a candle. A sheet of drawing-paper is prepared by having a little prepared hartshorn, (i. e., finely divided calcined

bones), well rubbed into its surface by means of a coil of list, or woollen cloth, the excess being gently wiped off by a cloth.

11. *Composition for Moulds.*—Moulds, formed from a composition of sulphur and iron scales, have been successfully employed in America in place of those usually made of brass, enduring the necessary pressure without injury. They are formed by dissolving the pulverised iron scales from a smith's forge in a melted sulphur: the proper proportions may be readily found by experiment. The compound is easily fusible, and takes very sharp and accurate casts from the originals.—*Tech. Rep.* i. 446.

12. *Cement.*—The following is a very excellent cement for the use of turners and artisans in general. The receipt is due to Mr. S. Varley: 16 parts of whiting are to be finely powdered and heated to redness, to drive off all the water. When cold, it is to be mixed with 16 parts of black resin, and 1 part of bees-wax; the latter having been previously melted together, and the whole stirred till of an uniform consistence.—*Tech. Rep.* i. 416.

13. *Hyalograph.*—M. de Clinchamp, teacher of the youth intended for the marine service at Toulon, in France, has invented an instrument called a Hyalograph, which takes off, with extreme correctness, the appearances of natural objects. A particular sort of ink serves to impress on paper designs taken on the glass of the instrument; and as the first impression cannot be erased, many proofs may be taken off, by inking it over again. The hyalograph serves also for many mathematical applications. The works executed with hyalographic ink have the appearance of lithographic designs.

14. *Blaek Lead Pencils.*—Hard black lead drawing pencils are made by Mr. Varley and Mr. Banks, by melting together fine Cumberland black lead in powder and shell lac. This compound is repeatedly powdered and re-melted, until of uniform composition; is then sawn into slips, and mounted as usual. Pencils thus made are uniform, and of great strength, and there is no waste of material.—*Tech. Rep.* i. 286.

ii. Chemical Science.

§ 1. *Chemistry.*

1. *On the Chemical Phenomena of thin Plates.*—Sig. A. Fusinieri has inserted an account of a long series of experiments on the chemical action which takes place in very thin layers of various substances. The fixed and volatile oils, alcohol, ether, balsams, *&c.* were placed on glass, water, and mercury, in the air, *in vacuo*, in different gases, and in different positions, and the phenomena, carefully observed. These are detailed in the *Giornale di Fisica*, iv. pp. 133, 209, 287, and are to be continued; but the following are the general principles deduced from the three first parts of the mémoir.

1. All liquid substances reduced to uniform laminæ, so thin as to decompose light by reflection and transmission, have the power of throwing off their own substance in the direction of the angle of the wedge parallel to the plane, which passes by the angle perpendicularly to the thickness.

2. This expulsive force is in the same substance always less as the wedge is more acute, and greater as the wedge is less acute, within certain limits not yet determined.

3. From this force proceeds the spontaneous expansion of liquid masses reduced in some parts to their cuneiform plates, and continued in that direction (1), if the movement be not impeded by external obstacles. In this way drops which form themselves into spherical segments on horizontal surfaces that have no attraction for the drops, and admit of free motion, expand in all directions.

4. If the expansion is impeded, not by external obstacles placed in the line of its direction, but by some other force which obliges the wedge-shaped lamina to preserve its form, then, by the action of the same force, the substance is successively withdrawn, and extended in the same direction.

5. When the obstacles to the motion are opposed to the angle of the wedge, then the force re-acts in a direction normal to the resistance, and therefore nearest to the primitive direction, producing expansion in the new direction.

6. This force of expansion is smallest in water and aqueous solutions, much greater in all combustible liquids, and among these greater with the odorous than the inodorous substances. It is greatest of all in the odorous and volatile substances. In the sulphuric and nitric acid it exists in a degree equal to that of the volatile combustible liquids.

7. The vapours which arise from the laminæ of volatile liquid combustibles, if retained in contact with their respective substances, impede the expansion of re-action in the direction normal to the plane of the lamina (5).

8. The force of expansion spoken of is essentially distinct from that by which bodies tend to form vapour: for the degree of this force has no relation to the degree of evaporability, and sometimes has a contrary ratio.

9. This force has, in common with the forces of electricity and magnetism, the power of increasing its proper action in the direction of the angles, of rendering it more intense, and of determining a current of matter in that direction.

10. Finally, The effects of this force are not limited to mere expansion, but consists also in chemical changes, decomposition taking place, and re-compositions being produced.

2. *Effect of Heat on the colour of the Ruby.*—In subjecting rubies to high degrees of heat, Dr. Brewster observed a very singular effect produced during their cooling. At a high temperature the red ruby becomes green; as the cooling advances, this green tint gradually fades and becomes brown, and the redness of this brown tint gradually increases till the mineral has recovered its primitive brilliant red colour. A green ruby suffered no change from heat, and a bluish green sapphire became much paler at a high heat, but resumed its original colour by cooling. *Edin. Journ.* vi. 379.

3. *Pyrometer.*—Mr. Sivright, proposes to remove the objections to Mr. Wedgewood's pyrometer, in consequence of the irregular contraction of the clay-pieces used, by substituting for them pieces of agalmatolite, or figure-stone of China. In making experiments on this substance, he has found it capable of contracting in its dimensions considerably, by heat, without any other change, and he considers that its use will be advantageous.

4. *Source of Cadmium.*—Mr. W. Herapath states, that he has obtained cadmium in abundance from the zinc works near Bristol. Zinc is obtained by putting calamine with small coal into a crucible, which being closed at top, has a tube proceeding through its bottom, into a vault below; beneath the tube is a vessel of water, and a short tube is at hand to connect at a proper time with the long one, so as almost to reach the water. The workmen do not connect until the "*brown blaze*" is over, and the "*blue blaze*" begun. This brown flame is owing to cadmium, the oxide of which attaches itself to the roof of the vault, in greatest quantity just over the orifice. It is mixed with soot, sulphuret of cadmium, and oxide of zinc. Some portions contain from twelve to twenty per cent. of cadmium.

The metal is obtained by dissolving this substance in muriatic acid, filtering, evaporating to dryness, re-dissolving and filtering, and precipitating by a plate of zinc. The cadmium thrown down is to be mixed with a little lamp-black or wax, put into a black

or green glass tube, and placed in the red heat of a common fire, until the cadmium has sublimed into the cool part of the tube; then the residuum is to be shaken out, which is easily done without loss of cadmium: a little wax introduced into the tube, and a gentle heat applied, the metal melts, and by agitation forms a button.

Mr. Herapath thinks that the zinc smelter, if he were to put up his tube earlier, and collect the first few pounds of metal separate, would be able to collect abundance of cadmium, and afford it at a cheap rate for the purposes of the arts.—*Annals of Phil.* iii. 435.

5. *Crystallised gold.*—When a solution of gold in ether is left for a considerable length of time, the gold is gradually reduced, and deposited in the metallic form, and crystallized.

6. *Stanniuretted Hydrogen.*—Professor Kastner has observed, that when tin is treated with moderately strong muriatic acid, the hydrogen gas developed is combined with tin. It has a very particular and penetrating odour: when compressed into water, a considerable quantity is dissolved; when burnt it produces a blue light, and gives off white oxide of tin in fumes. When introduced into a very weak solution of gold, purple of Cassius is instantly formed; and, applied in this way, the gas is a highly-sensible re-agent for the minutest portion of the noble metal. Bismuth, when similarly treated, forms a similar substance. Both these gases considerably resemble telluretted hydrogen.

It may be observed, that, when treated in the same way, zinc also produces a gas, containing large quantities of the metal when recent.

7. *Deutoxide of Copper.*—M. Berzelius says, "the deutoxide of copper attracts the humidity of the atmosphere very rapidly: it is reduced so rapidly in hydrogen gas, that if a piece be highly heated, but not red, and plunged into a bottle of the gas, the oxide takes fire and is reduced, and water trickles down the phial. According to the weight lost in this reduction, it appears to be composed of

Copper	.79825	100
Oxygen	.20175	25.272
	1.00000	

Ann. de Chim. xvii. 26.

8. *Preparation of Kermes Mineral.*—M. Hensmans recommends, that in the preparation of this substance, the sulphuret of antimony, should be fused with caustic potash, and not with a carbonate: for then, when a solution is made, and a current of carbonic acid

passed through it, abundance of kermes falls down; and on the addition of diluted sulphuric acid to the solution so treated, a second portion of the golden sulphuret is obtained: both these preparations have a fine and beautiful appearance.

9. *Jodo-cyanuret of Potassium and Mercury.* When solutions of prussiate of mercury, and hydriodate of potash, are mixed, abundance of crystals fall down, which, when collected, washed, and re-crystallized, appear in large thin brilliant plates unalterable in the air, without smell, but in solution having the odour of bitter almonds; soluble in 16 times their weight of cold water, and in much less when hot; soluble in 96 parts of alcohol. Their composition has not been determined, but 24 parts gave, with hydro-sulphuret of soda, 13 parts of sulphuret of mercury.—CAILLOT. *An. de Chim.* xix. p. 280. *(See our Foreign Science.)*

10. *Hydriodide of Carbon.* In the Philosophical Transactions, for 1821, I have described a compound of chlorine and olefiant gas, but had not at that time the means of ascertaining its composition. Since then, I have obtained it in greater quantity, and analyzed it. Four grains were passed in vapour over heated copper, in a green glass tube; iodide of copper was formed, and pure olefiant gas evolved, which amounted to 1.37 cubic inches. As 100 c. i. of olefiant gas weigh about 30.15 grs., so 1.37 c. i. will weigh 0.413 of gr. Now 4 grains minus 0.413 leaves 3.587 iodine, and 3.587 : 0.413 : : 117.75 : 13.55 nearly. Now 13.55 is so nearly the number of 2 proportions of olefiant gas, that the substance may be considered as composed of

1 proportion of iodine	117.75
2 proportions of olefiant gas . .	13. 4

and is therefore analogous in its constitution to the compound of chlorine and olefiant gas, sometimes called chloric ether. *M. F.*

11. *Formic Acid, and Formate of Lead.* The following estimations of the composition of these bodies is given by Dr. Gobel from careful experiments:

Formate of lead	1 atom oxide of lead . .	107.5
	1 —— formic acid . .	34.9
	1 —— water	8.45

Formic acid			
Carbon . . .	11.35,	or 2 atoms oxide carbon . .	26.4
Hydrogen . .	1.06,	1 —— water	8.45
Oxygen . . .	22.43		
	34.84		34.85

Annals Phil. iii. 463.

12. *Dilated Caoutchouc Bottles.*—Mr. Forster has shewn, that caoutchouc bottles may be expanded, until so thin and large as to answer many purposes of bladders, in philosophy and the arts. His process is to condense air into the caoutchouc which gradually forms the appearance of a blister at the bottom of the bottle, and which, extending, at last includes the whole. In this way the bottles have been made 6 and 7 inches in diameter; and by care, Mr. F. thinks may be made thin enough for balloons. When rubbed on warmed paper, these globes become very strongly electrical.—*Phil. Mag.* lix. 263.

13. *Prize Question.*—The Royal Academy of Sciences has proposed the following prize subject for 1824.

1. To determine, by multiplied experiments, the density acquired by liquids, and particularly by mercury, water, alcohol, and ether, under pressure equal to the weight of many atmospheres.

2. To measure the effects of the heat produced by these compressions.

The prize is a medal of gold, value 3000 francs. The memoirs to be sent in before January 1, 1824.

§ 2. Electricity, Magnetism.

14. *Pyro-Electricity of Minerals.*—M. L'Abbé Haüy has remarked, with regard to the electricity produced in certain crystals by an alteration of temperature, that it is of two kinds. The accidental circumstances which led to the discovery took place whilst he was examining some crystals of the oxide of zinc, from Limbourg, near Aix-la-Chapelle, and fragments of the acicular variety of the same mineral from Brisgau. Having placed a piece of one of these substances in a very cold window for a few moments, it was found on examination to be electrical. Its poles were ascertained, and the mineral then placed in a milder temperature, when the electricity soon became nul; but being approached to a fire-place, the power was greatly renewed, but the poles were inverted.

These results have been verified by M. Haüy on other crystals, especially those of the tourmaline. In taking them for examples, he has endeavoured to bring under one point of view all that passes with respect to them in the interval comprised between the limits of temperature, beyond which the electric action disappears without return. He has given the name of *ordinary electricity* to that produced by heat, and *extraordinary electricity* to that produced by cold. If, therefore, commencing at the point where the excess of heat destroys in the tourmaline the effects of the ordinary electricity, that mineral be left to cool it will soon give signs of ordinary electricity. The action of

the poles at first feebly augments to a certain degree, beyond which it gradually diminishes, and at last disappears. With a temperature a little lower, however, the extraordinary electricity appears, and the poles resume their power, but in an inverted order, so that the pole at first positively electrified becomes negative, and the negative pole becomes positive.

These results are detailed at large in M. l'Abbé Haüy's *Traité de Physique.* We have taken these from the *Annales de Mines.* vi. p. 608.

15. *Electricity of the Atmosphere.*—M. Bourdet, an ex-captain in the French service, has described, in a letter, a very singular electrical phenomenon which he witnessed in Poland, Dec. 24, 1806. The weather, according to the Poles, had never been milder at that season of the year, no snow had been seen, nor had the usual cold weather of the north, which generally sets in early in that country, then commenced. Rains and storms, however, were frequent. "I was," says M. Bourdet, "with the advanced guard of light cavalry; the commander gave me an order to halt in the rear and see that my guns were disembarrassed, and then to rejoin, as quickly as possible, the light brigade. In spite of the efforts of my men, the guns were not cleared from the marshy ground in which they were entangled, without great labour. We were advancing across the field about nine o'clock in the evening, when a strong gust of wind suddenly arose, (the sun had shone brightly during the day,) and in a few minutes after, the night became so dark, that we could not see the heads of our horses. The wind blew so violently, that the horses stopped. At that moment the extremity of the hair on their ears became luminous, as well as all the longer hairs on their bodies, except the locks on their manes and tails. All the metallic extremities of their harness, and all the metallic sharp points of the carriages of our guns, were studded with luminous points, so that one might have supposed, had it been spring, that a swarm of glow-worms had covered our horses and guns. Our quarter-master observed that the points of my mustachios were luminous. The same phenomenon was seen on some of the cannoneers, but none of us had our eye-lashes or hair rendered luminous. These lights remained as long as the gust of wind lasted, namely, for three or four minutes. Their colour was a soft violet, and they terminated in a bright white. The horses held their heads high, their ears were erect and moving, their nostrils open and respiring, their manes and tails erected, their fore legs thrown forward, and their hind ones back. Their attitude, in general, was that of animals seized with terror. During the time the wind blew, they remained at full stop, and when feeling the spur, some stood stock still, and others kicked, as if they had been reluctant to advance. When the

wind ceased, the lights disappeared, and a deluge of rain, mingled with hail, fell. But though the obscurity continued, our horses moved on, shaking themselves at times, panting forcibly, and neighing; but they continued on their march. On arriving at the advanced post, I mentioned to my comrades the phenomenon we had witnessed, and though they had been only three leagues from us, they had felt no wind, but experienced much rain. The wind we encountered had an opposite direction to the rain."

16. *Electrometer for Minerals.*—M. Razoumansky describes an electrometer, which is far more sensible than M. l'Abbé Haüy's, though the only difference is that the needle he uses on the pivot is of steel, and is magnetized. By trials with gems and stones, the instrument is said to have been affected more readily than the Abbé's, and without requiring that attention to temperature which M. Haüy directs. M. Razoumansky attributes its excellence to its being magnetic; but it is by no means evident how the magnetism should improve it, since the electricity in the state in which it exists on the rubbed or otherwise treated gems, would have no influence on that power.—*Journal de Phys.*, xciii. p. 398.

17. *Remarkable Luminous Phenomenon.*—M. Doe, being surprised by the night, whilst in the forest near Boulancourt, in the neighbourhood of Brienne, department de l'Aube, was witness to a very brilliant luminous phenomenon which took place in a neighbouring marsh. The appearance began about two o'clock in the morning, apparently at one of the openings of the wood on the western side, whilst the sky was serene, the stars brilliant, and the air calm and temperate. The fire burnt quietly, without jets or undulations, in the form of a quadrangular pyramid. The colour of the flame was a pale red, verging on white, and the appearance of the whole was compared by M. Doe to sun-set, when it takes place behind a red cloud, girt by a dark zone. On proceeding to the place where this phenomenon had its rise, it was found to be a marsh about half a league in extent, traversed by ditches, which furnished the phosphoric matter of the flame. The greatest height of the luminous meteor was ten or twelve feet; there was no heat, but the light was bright enough to permit of reading by it. In about half an hour the effect was considerably diminished, and at the expiration of an hour it had entirely ceased.—*Journal de Phys.*, xciii., 236.

18. *Terrestrial Magnetism.*—The following are extracts from a letter written to M. Rumker by Professor Hanstein, containing a short account of his own discoveries.

" I observed here at Christiana, in the months of November

and December 1819, and in March, April, and May, 1820, seven or eight times every day, the time of 300 oscillations, (performed by a magnetic steel cylinder, suspended by a very fine silk thread,) by which I have found :

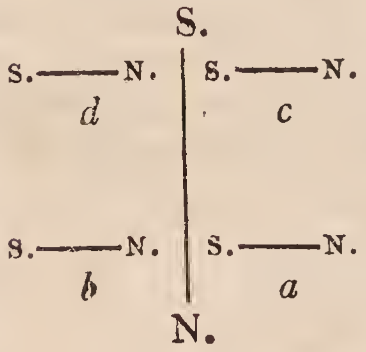
First, That the magnetic intensity of the earth is subject to a diurnal variation, so that it decreases from the first hours of the morning till about ten or eleven, when it arrives at its minimum ; from that time it goes on increasing till four in the afternoon, and in the latter months till six or seven in the evening. This force afterwards decreases anew during the night, and about three in the morning reaches its maximum, when it again returns, by little and little, to its minimum about ten or eleven in the morning, and so on continually.

Second, That whenever the moon passes the equator, the magnetic intensity is considerably weaker in the two or three following days.

Third, That the magnetic intensity is still more reduced during the appearance of an aurora borealis, and is so much the weaker as this meteor is extensive and powerful. The common intensity returns only by degrees, and 24 hours afterwards.

Fourth, That the magnetic intensity appears to have a very considerable annual variation, being stronger in the winter months than in the summer months.

" My second magnetic discovery is the following:—I have found that every vertical body S. N. whatever it be, and of whatever kind of matter has a north pole at bottom, and a south pole at top, as in all vertical bars of iron, for otherwise it would be impossible to explain the phenomena which I have observed, and which I have determined by a great number of incontestible experiments, namely, that the magnetic cylinder oscillates more quickly towards the north in *a*, and more slowly towards the south in *b*. And, on the contrary, it oscillates more slowly towards the north in *c*, and faster towards the south in *d*. I have found this law constantly confirmed by my experiments near the walls and partitions of houses, whether of wood or stone, and even near large trees in the garden. This action must necessarily exert its influence on the direction of the compass-needle on ship-board. The whole mass of wood in a ship has in this way a magnetical axis, and the observed variation of the compass ought rather to be attributed to this influence than to that of iron, guns, and ballast carried by the vessel. Hence it results that all observations on the mag-



netic intensities made within doors are uncertain.—*Zach's Correspondence.*

19. *Intensity of the Magnetic Force in different parts of the World.*—The following table is the result of Professor Hanstein's laborious observations.

Places.	Diff.	Intensities.
Peru	0° 0′	1.0000
Mexico	42 10	1.3155
Paris	68 38	1.3482
London	70 33	1.4142
Christiana	72 30	1.4959
Arendahl	72 45	1.4756
Brassa	74 21	1.4941
Hare Island	82 49	1.6939
Davis' Straits	83 08	1.6900
Baffin's Bay	84 25	1.6685

Zach's Correspondence.

20. *Magnetism affected by Earthquakes.*—M. Arago has transmitted to the French Academy of Sciences, an account of an observation he had made, which proves that the recent earthquake, the shocks of which were felt at Lyons and its neighbourhood, also extended its action to Paris. M. Arago has an observatory at Paris, for the purpose of observing the variations of the magnetic needle. On the 19th February, the needle remained perfectly steady until half-past eight o'clock; at a quarter before nine, it became agitated in a very extraordinary manner, with an oscillatory motion in the direction of its length. On observing this truly singular phenomenon, M. Arago was of opinion that it was occasioned by an earthquake.

At the same day and hour, M. Biot remarked an oscillating movement produced by the same earthquake, at his own residence in the College de France.

iii. Natural History.

§ 1. *Mineralogy, Geology, Meteorology.*

1. *Analysis of Yellow Copper Ore, by Mr. Phillips.*—Mr. Phillips has lately analyzed the different varieties of yellow copper ore, and has arrived at the conclusion that they are all of similar composition. His process was, after pulverizing the ore, to heat it in nitro-muriatic acid until the sulphur was acidified, filter, precipitate by nitrate of barytes, and collect the sulphate; then separating the excess of barytes from the solution by sulphate of

soda, to add to the clear solution ammonia, and collect the oxide of iron; the ammoniacal solution was then heated with potash, so as to drive off the ammonia, and the copper thrown down collected. By the appropriate tests, lead and arsenic were ascertained in the ore, if present.

Guineveau has analysed the amorphous yellow copper ore. The varieties analysed by Mr. Phillips are the crystallised and the mamellated. The composition of the three kinds are as follows;

	Amorphous.	Crystalysed.	Mamellated.
Sulphur	36.36	35.16	34.46
Iron	33.00	32.20	30.80
Copper	30.64	30.00	31.20
Earthy matter	0.0	0.50	1.10
Lead arsenic & loss	0.0	2.14	2.44
	100.	100.	100.

The mamellated variety contained no lead.

A compound of two atoms proto-sulphuret of iron, and four of per-sulphuret of copper, would consist of

4 atoms sulphur	64, or	34.78
2 —— iron	56, --	30.44
1 —— copper	64, --	34.78
		100

If, Mr. Phillips observes, the lead arsenic and earthy matter be neglected, and its place be supplied by copper, then the composition of the crystalline variety will approach nearly to, and that of the mamellated variety be almost exactly, that of the atomic composition.—*Ann. Phil.* iii., 301.

2. *Native Carbonates of Manganese.*—The carbonate of manganese of Nagiac contains, according to M. Berthier,

Quartz	.210
Protoxide of manganese	.443
Lime	.043
Carbonic acid	.304
	1.000

Or, removing the quarts,

Protoxide manganese	.560, or	Carb. manganese	905
Lime	.054	Carb. lime	095
Carbonic acid	.386		1.000
	1.000		

Carbonate of manganese from Freyberg gave,

Protoxide of manganese	.510, or	Carb. manganese	.822
Ditto of iron	.045	Carb. iron	.073
Lime	.050	Carb. lime	.089
Magnesia	.008	Carb. magnesia	.016
Carbonic acid	.387		1.000
	1.000		

Ann. des Min. vi., 594.

3. *Native Chromates of Iron.*—The following is the composition of two chromates of iron, analysed by M. Berthier, the first from St. Domingo, the second from Baltimore.

Peroxide of iron	.370	.350
Oxide of chromium	.360	.516
Alumine	.215	.100
Silica	.050	.030
	.995	.996

Ann. des Min. vi., 577.

4. *Hydrate of Alumina.*—A native hydrate of alumina, from Beaux, department des Bouches-du-Rhone, analysed by M. Berthier, gave,

Alumine	.520
Water	.204
Peroxide of iron	.276
Oxide of chrome, a trace	
	1.000

M. Berthier considers the oxide of iron as merely mixed with the mineral; its true constitution therefore is,

Alumine	.72
Water	.28

The mineral occurs in pieces of indeterminate form, and sometimes in round grains, agglutinated by a paste of the same nature, penetrated by carbonate of lime. The ferruginous matter is of a blood-red colour, its fracture uniform and shining, never radiated, its specific gravity small and variable.—*Jour. de Min.* vi., 531.

5. *Native Nitrate of Soda.*—An immense quantity cf nitrate of soda has been discovered in Peru, in the district of Jarapaca, a short distance from the frontiers of Chili. It forms a bed many feet in thickness, which in many places appears at the surface of the soil, and occupies an extent of forty leagues. The salt is found sometimes effloresced, sometimes crystallised, but most frequently mingled with clay and sand. It is bitter, and deliquescent; it contains a little sulphate of soda.

The site of the mineral salt is about three days journey from Conception, a port of Chili, and from Iquiqui, a port of Peru.

Above 6000 quintals of the purified salt have been taken to these parts for the purpose of trade. It may be very advantageously used in the production of nitric acid.

6. *Recent Iron Pyrites.*—Professor Meinecke observed, on the Jolauer Heath near Halle, tables of an inch in breadth of iron pyrites, intermixed with reeds, and which he observed continued to increase in size; thus proving their new formation.

7. *Remarkable Glaciers, containing Organic Remains.*—Mr. Otto von Kotzebue, in his Voyage to the South Sea and Behring's Straits remarked a singular formation of ice in the western part of the gulf which he discovered to the north of Behring's Straits, and to which he gave his name.

On the 4th of August, having ascertained the longitude (161° 42′ 20″ W. of Greenwich) and latitude (66° 13′ 25″ N.) of the place where they had cast anchor, they left the Rurick, with two boats provided with several days' provisions. In the place where they passed the night, the earth rose rapidly from the flat shore to a height of 120 feet, and then appeared as an immense plain, covered with moss, a little herbage being found on the inclined surface. An English mile from the shore the depth of the sea was only five feet.

In the place where they passed the fourth night, the rise of the surface from the shore was not great, but after that it attained a considerable elevation, abundance of herbs being found at the bottom, and above, moss.

On the 8th, a tempest prevented their return to the ship, but gave occasion to the remarkable discovery made by Dr. Eschholz, whose name was given to the bay.

" We had travelled considerably in various directions, without perceiving that we were upon glaciers. The doctor having made an excursion to some distance, arrived at a ruin of the mountain, when, to his surprise, he discovered that the whole of its interior was pure ice. Immediately we all went to the spot, provided with tools. We soon came to a place where the shore rose nearly perpendicularly above 100 feet, and then continued to extend with a slight inclination upwards. Here we saw masses of the purest ice above 100 feet high, which were preserved under a carpet of moss and herbs. They must have been formed by some terrible convulsion. The portion which had fallen down being exposed to the sun and air, melted, and much water ran from it into the sea.

" An indubitable proof that the ice before us was primitive ice, is afforded by a great number of the bones and teeth of the mammoth, left when it is melted. I found one very fine tooth. We could not explain to ourselves the cause of a very strong odour, which exhaled from this country, similar to that of burnt horn.

The soil of these mountains, which, to a certain height, is covered with abundant herbage, is only half a foot thick. It is a mixture of argillaceous earth, sand and earthy matter. The ice gradually fuses below the verdure, has its position changed, and continues growing in a lower position, and in this manner one may conceive it possible that, in a long series of years, the mountain may disappear, and a verdant valley occupy its place."

By observation we found the latitude of this tongue of earth 66° 15″ 36′ N. At the bottom of the eastern part of the Bay of Eschholz, high mountains were perceived.

8. *Cave containing Rattlesnake Skeletons.*—About the year 1748, some labourers in working a quarry in the neighbourhood of Princeton, in America, for the stone with which the college is built, discovered a small cavern, which contained the entire skeletons of an immense number of the rattlesnake. The bones were in such quantity as to require two or three carts for their removal. There can be little doubt that this cavern had once a small opening, which was afterwards closed by the accidental fall of a stone, or some other impediment. This cave had probably been the winter abode of the rattlesnake for years, where many had died through age, and others in consequence of the circumstance just mentioned. Such facts as these have gained an interest since Mr. Buckland's description of the cave at Kirkdale, from the relations that are evident between them.

9. *Meteors.*—A very large brilliant meteor passed over the city of Richmond (Virginia), on the 16th March, about five minutes after ten P.M., in a direction from the north-east to the southwest. It is described as being nearly as large as a barrel, emitting sparks in every direction, and leaving behind it a large train of light; some even thought they heard a hissing noise as the meteor passed over them. The light was bright, silvery, and very powerful. It exploded with a loud noise, leaving a wide stream of fire, which was visible for several minutes.

On the 9th of April, about nine in the evening, a large meteor was seen at the town of Rodiz in Avignon. It appeared like a pillar of fire, sending out an infinite number of sparks. After some seconds it disappeared, and a loud explosion was heard.—*Phil. Mag.* lix, p. 399.

10. *Analysis of the Meteorolite of Juvenas.*—A meteoric stone fell at Juvenas on the 15th of June, 1831, of which a short notice was given in a former number of this Journal. An analysis of it has been made by M. Laugier, who finds it to accord with that from Jonzac, in containing no nickel, and in containing chromium. He therefore urges it as a further proof of the propriety of considering chromium as the distinctive metal of aërolites, at least till one or more of them has been found without it.

The stone from Juvenas resembles other meteoric stones in its aspect, but is more friable than most of them. It contains no globules of iron sensible to the pestle, but small crystals may be seen in it, having an evident cleavage, which have received the name of feldspar, and appear by analysis to be that substance. Acids only acted on it partially, but after fusion with potash, its elements, by further treatment, readily separated from each other. They are given as follows:

Silica	40.
Oxide of iron	23.5
Oxide of manganese	6.5
Alumine	10.4
Lime	9.2
Chromium	1.0
Magnesia	0.8
Sulphur	0.5
Potash	0.2
Copper	0.1
Loss from unknown causes	4.8
—— indispensable	3.0
	100.0

The circumstance of a constant loss, rather than an increase of weight, induces M. Laugier to conclude that the iron and magnesia are in the state of oxides in the stone. When powdered, no part of it was attracted by the magnet. The loss, however, remains unaccounted for. When a portion of the stone was distilled it lost in weight, but M. Laugier could not satisfy himself of the substance gone off; it was neither water nor carbonic acid gas.

The analogy of this aërolite to that of Jonsac is remarkable: the entire absence of nickel, the disappearance almost complete of the sulphur and magnesia, with the presence of an abundant quantity of lime and alumine, establish a considerable difference between these two stones and those previously known to have fallen from the air.

Reference is then made to several other aërolites which fell the 13th of December, 1813, in the neighbourhood of Lantela, in the government of Wibourg in Finland. Mr. Nordenskiold, who examined them, describes them as very friable, containing grains of olivine, and also amphigene, scarcely at all attracted by the magnet, and containing no nickel.

Considering the difference in aspect and composition between these three stones and those which were previously known, M. Laugier thinks that these substances may be arranged in two classes, but he does not insist much on this division.—*Annales de Chim.* xix. 264. *(See our Foreign Science.)*

11. *Volcanoes in Iceland.*—Violent volcanic eruptions took place last winter in Iceland, where they were wholly unlooked for. The mountain called Oefields-Jokkeler, to the south-east of Hecla, which had been quiet ever since the year 1612, broke out with great fury on the 19th, 20th, and 21st of December, 1821, so that the ice, with which it was covered, burst with a tremendous crash. The earth trembled, and enormous masses of snow were precipitated from the summit (a height of 5,500 feet) into the plain. From that time, a column of fire continued to rise from the crater, which ejected vast quantities of ashes and stones, some of them weighing from 50 to 80 lbs., half calcined, were thrown to the distance of five English miles from the crater. It does not appear that any great damage has been done by this eruption; the mass of sulphureous ashes which covered the adjoining country like a thick crust, has since been removed by a violent storm and torrents of rain. The mountain continued to burn till the 1st of February, and smoked till the 23d, but the ice had again formed round the crater. During the eruption, the weather in the island was extremely unsettled and stormy, with loud noises and sensible shocks as of an earthquake.—*New Monthly Mag.*, vi. 217.

12. *Volcanoes in the North.*—Two volcanoes have broken out in the North, at the eastern boundary of Asiatic Russia, viz., at the western extremity of North America. The inhabitants of the Aleutes Isles (or Foxes) perceived, in the night of the 2d of March, 1821, all the signs of a great natural commotion. A strong S.W. wind, violent shakings of the earth, and subterraneous noises; immediately after the atmosphere was inflamed, and sand, with cinders, fell during the whole night. While this was passing at Unalachka, another volcano burst forth at Ounimach, an island about 24 leagues from Unalachka. Columns of smoke and fire rose from the latter place until the month of August. The land has been much altered by these commotions, the sea having receded to a considerable distance.

§ 2. Medicine, &c.

13. *Proper State of Prussic Acid for Medicinal Use.*—A series of experiments have been undertaken by a company of associated physicians, surgeons, and naturalists at Florence, to determine the best state of the hydro-cyanic or prussic acid for medicinal purposes. The experiments were made with great care, and varied several ways. Different preparations of the substance were used, rabbits being the animals on which they were tried. Their joint opinion is expressed as follows: "We may then conclude, from our researches, that the essential oil of the prunus laura-cerasus is to be preferred in medical practice to all other prepara-

tions which contain the hydrocyanic acid; for, unlike the distilled water of the plant and pure prussic acid, it contains the same proportion of the acid, and is of the same power, whether recently prepared or old, when made in one place or another, after exposure to the air, to light, or to heat. We think also that the oil of olives, or of almonds, is the most proper vehicle in the proportion of an ounce to 12 drops of the essence, or in a smaller dose when employed by friction externally."

14. *Oil of Turpentine rendered palatable.*—Dr. Nimmo employs the following method to purify the oil of turpentine for medicinal use, "without diminishing its efficacy, but greatly lessening its disagreeable taste, and its injurious effects upon the kidneys." To 8 parts of the oil add 1 part of the strongest alcohol, and let them be well agitated. In a few minutes a separation takes place, the oil, unless very impure, falls to the bottom, and the alcohol having discharged the impurities, floats at the top. Pour off the alcoholic portion, add an equal quantity of the alcohol as before, agitate and separate the liquids. If this be repeated three or four times the oil becomes nearly tasteless, almost without smell, and when a portion of it is evaporated, it leaves no residuum. It is necessary to remark, that pure as the oil may be rendered, it speedily undergoes alteration, and returns to its original state of greater or less impurity. *(See Dr. Nimmo's Paper, in our last Number.)*

15. *Hooping-Cough.*—Dr. Archer, an American physician, announces that the hooping-cough is cured by vaccinating the patient on the second or third week after the commencement of the disease. This is a singular discovery, and if the result be doubted, the experiment is at least harmless.

16. *Large Human Calculus.*—A large human calculus has been described by Professor Cumming, of Cambridge: it weighs 32 ounces, and measures 15½ inches in circumference. Its specific gravity is 1.756. The nucleus is lithic acid, and to this succeeds a considerable portion of the oxalate of lime, then layers of the triple phosphate, covered by a thick coating of lithic acid, the external surface being composed principally of the fusible calculus. It is in the possession of Trinity College. A calculus is also noticed from the intestines of a hare: It is composed of vegetable matter and the phosphates.

17. *Physiological Prize Question.* The Royal Academy of Sciences have proposed, as a prize question for 1823. "To determine by precise experiments, the causes, either chemical or physiological, of animal heat." It is required that the heat emitted by a healthy animal in a given time, and the quantity of car-

bonic acid produced, be exactly determined; and that the heat be compared to that produced by the combustion of carbon, whilst forming the same quantity of carbonic acid. The prize is a gold medal of 3000 francs value. Memoirs to be sent in before Jan. 1, 1823.

18. *Prize Questions, College of Surgeons.*

Jacksonian Prize.—Not either of the prizes for 1820 having been adjudicated, three prize subjects are proposed for the year 1822; viz. Injuries and Diseases of Muscle, Diseases of the Skin, and the Diseases of the Rectum.

Candidates are to be members of the College, and the dissertations are to be written in English, and the number and importance of facts will be considered principal points of excellence. Each dissertation is to be distinguished by a motto or device, and accompanied by the name of the person, sealed up, but distinguished by the same motto. They are to be sent to the secretary before Christmas-day, 1822.

ART. XIX.—METEOROLOGICAL DIARY for the Months of March, April, and May, 1822, kept at EARL SPENCER'S Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern spect, about five feet from the ground, and a foot from the wall.

For March, 1822.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Friday	1	24	44	30.12	30.04	SW	SW
Saturday	2	40	50	30.04	30.08	NW	SW
Sunday	3	42	52	30.10	30.04	SSW	SSW
Monday	4	40	52.5	29.94	29.78	SE	SSW
Tuesday	5	42	52	29.78	29.61	WbS	W
Wednesday	6	45	53	29.30	29.37	SW	SW
Thursday	7	37	50	29.36	29.30	W	W
Friday	8	32	46	29.47	29.20	W	W
Saturday	9	37	51	29.42	29.50	W	WbS
Sunday	10	47	55	29.38	29.36	WbS	W
Monday	11	36	48	29.52	29.81	W	W
Tuesday	12	30	50	30.14	30.18	WbN	S
Wednesday	13	33	49	30.07	29.83	S	SbE
Thursday	14	40	53	29.72	29.87	SW	WbS
Friday	15	27	52	30.03	30.03	WbS	S
Saturday	16	32	52	30.00	29.90	SE	SE
Sunday	17	43	50	30.06	29.90	SW	WbS
Monday	18	44	51	29.98	30.20	NW	WbS
Tuesday	19	44	57	30.06	30.10	WbS	W
Wednesday	20	49	56	30.10	30.10	W	W
Thursday	21	42	58	30.10	29.97	W	W
Friday	22	39	53	30.00	30.18	WbS	WbS
Saturday	23	35	57	29.99	29.81	SW	WbS
Sunday	24	42	50	29.45	29.49	WNW	SW
Monday	25	33.5	48	29.51	29.53	SW	W
Tuesday	26	35	55	29.80	29.87	W	W
Wednesday	27	43	54	30.00	29.93	SW	SW
Thursday	28	43	64	29.79	29.79	WbS	W
Friday	29	40	58	30.10	30.20	W	SW
Saturday	30	44	50	29.60	29.31	WbS	NW
Sunday	31	34.5	43	30.21	30.28	NW	NNW

For April, 1822.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Monday	1	35	48	30.27	30.21	N	NW
Tuesday	2	38	47	30.21	30.21	NW	NNE
Wednesday	3	39	48	30.25	30.19	NW	WbN
Thursday	4	38	50	30.10	30.04	NW	NW
Friday	5	39	53	29.95	29.87	WbN	NW
Saturday	6	38	51	29.79	29.73	WbN	NW
Sunday	7	36	50	29.86	29.85	N	ENE
Monday	8	32.5	50.5	29.90	29.90	NE	NE
Tuesday	9	36	46	29.90	29.96	NbE	NE
Wednesday	10	315.	44	29.99	29.94	NE	ENE
Thursday	11	31	43.5	29.94	29.91	E	E
Friday	12	36	44	29.67	29.60	ENE	E
Saturday	13	38	54	29.60	29.70	ESE	SSW
Sunday	14	41	59	29.84	29.84	SW	S
Monday	15	46	56	29.84	29.84	S	EbS
Tuesday	16	47	57	29.84	29.84	NNE	NW
Wednesday	17	40	54	29.80	29.68	SSW	NW
Thursday	18	40	53	29.59	29.51	W	NW
Friday	19	41	60	29.54	29.57	W	NW
Saturday	20	44	61	29.53	29.50	WbS	SSW
Sunday	21	42	58	29.44	29.30	S	SE
Monday	22	44	55	29.22	29.20	SW	SE
Tuesday	23	37	52	29.18	29.26	E	WSW
Wednesday	24	38	55	29.41	29.43	SE	SE
Thursday	25	44	54	29.35	29.41	SSW	SW
Friday	26	39	58	29.57	29.80	SW	WSW
Saturday	27	43	56	29.80	29.83	SE	NW
Sunday	28	36	61	30.08	30.08	ESE	SE
Monday	29	41	65	30.10	30.08	SE	SE
Tuesday	30	46	64	30.11	30.11	NE	E

For May, 1822.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Wednesday	1	41	65	30.19	30.18	E	E
Thursday	2	34	65.5	30.10	29.96	NE	SE
Friday	3	44	63.5	29.89	29.70	ESE	SE
Saturday	4	48	63	29.50	29.50	E	E
Sunday	5	45	60	29.56	29.69	ENE	NE
Monday	6	49	64	29.60	29.60	EbN	NE
Tuesday	7	50	55	29.60	29.70	NE	EbN
Wednesday	8	43	52	29.81	29.84	NNE	NE
Thursday	9	33	50	29.69	29.49	E	NE
Friday	10	39	48	29.36	29.15	NE	SE
Saturday	11	38	61	29.40	29.53	ENE	E
Sunday	12	45	50	29.73	29.73	NE	NE
Monday	13	43	56	29.77	29.75	NE	NE
Tuesday	14	40	62	29.77	29.73	NW	WbN
Wednesday	15	42	71.5	29.73	29.76	W	W
Thursday	16	41.5	65	29.84	29.81	E	E
Friday	17	41	71.5	29.82	29.82	NE	EbN
Saturday	18	53	70	29.85	29.88	NW	NW
Sunday	19	47	71	29.95	29.91	NW	E
Monday	20	48	72.5	29.96	30.00	E	NW
Tuesday	21	46	73	30.10	30.13	NE	NE
Wednesday	22	49	64	30.20	30.18	NE	ENE
Thursday	23	45	61	30.17	30.08	ENE	NE
Friday	24	47	64	29.98	29.92	NE	ENE
Saturday	25	43	68	29.89	29.76	NE	ESE
Sunday	26	41	62	29.63	29.70	SE	WbS
Monday	27	41	67	29.90	29.90	SW	WSW
Tuesday	28	55	69	29.97	30.06	WbS	N
Wednesday	29	46	71	30.08	30.08	N	WbN
Thursday	30	41	67	30.17	30.10	W	W
Friday	31	48	72	30.09	30.10	WSW	W

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